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Art. VIII.—GAMA ON INJURIES OF THE HEAD.

Traité des Plaies de Tête et de l'Encéphalité, principalement de celle qui leur est consecutive: ouvrage dans lequel sont discutees plusieus questions relatives aux fonctions du systeme nerveux en generale. Par J. P. Gama, D.M., *Chirurgien in chef, premier professeur a l'Hopital militaire d'instruction du Val-de-Grace, &c. &c. &c. Paris,* 1830.

THE author of the work, the title of which we have given, holds a distinguished rank in the profession in France. Being an army surgeon, and a professor, as well as surgeon in chief, at the military school and hospital of Val-de-Grace, his experience has been ample; and being a colleague and a disciple of the French medical reformer, Broussais, he has attentively studied the physiology and pathology of the nervous system; while he has presented his practical knowledge in a scientific form much to his own credit, and, we trust, to the benefit of the public. In this treatise, devoted to the sequelæ of cerebral injuries, we discover a strong and judicious mind, operating on

intricate and difficult subjects and on the numerous unconnected and apparently opposing facts hitherto collected by the industry of practitioners; and, by endeavouring to reconcile difficulties and to bring order out of confusion, impressing its own character on the production before us. Little, perhaps, really novel, is presented, whether in fact or opinion: but the whole is judicious and valuable, as presenting the latest systematic work on injuries of the brain, as coming from the physiological school of medicine, as recording interesting facts, and as examining questions of great intricacy and importance in relation to the nervous system; while the author generally avoids extremes, and is usually satisfied in not pushing his deductions beyond the bounds to which his facts should restrain him. Exceptions to these remarks we shall have occasion to point out in the short analysis, or rather summary of the author's opinions, which we shall present—without undertaking to afford a complete view of the disputed questions, or even of the facts and arguguments deduced by the writer. For these we must refer to the book itself; our limits are too circumscribed.

Before entering on this analysis, we have one word to say on "physiological medicine," so called. This expression has been introduced by Broussais and his disciples, most fortunately for their success, and arrogated with much self-complacency to their own system—while all other physicians have been ridiculed as "ontologists," as believers in disease—being a separate ens, or being, superadded to the system, and, like some little demoniac, preying on one or more organs, or rapidly changing his place as caprice may indicate. We protest against all such representations; we have never known an ontologist; we doubt that any such exist, or even have existed among men of reputed science. All scientific physicians are indeed physiological physicians; and the main distinction between the present dogmatic teachers of new doctrines and their predecessors or opponents, consists, first, in the difference of their physiology, and secondly, in the support or denial of the opinion that all diseases are simply an increase or diminution of organic actions. That many are, cannot be doubted; but that many complaints are truly specific, that is, induced by a peculiar cause, and manifesting phenomena not deducible from the normal or physiological actions of the part, seems to us as little capable of dispute. We hate proscription in medi-

cine as in politics, and cannot regard the cause sound which requires its support. While we have a high respect for the principles of the new French school, and are sensible of their utility to the profession, especially in France, we are not its disciples, although we claim as anxiously as any, the title of physiological physicians, and disclaim any and all ontological notions. We have thought these explanations due to ourselves and the profession, especially when noticing, with commendation, as we frequently do, the productions of the Broussais school.

The present work is divided into chapters and subdivided into articles; the most important facts being illustrated by appropriate cases, detailed with the minuteness characteristic of the clinical schools of France.

Chapter I. *Wounds of the head restricted to the exterior parts*,—that is, to the bone and membranes, as well as the scalp. Wounds of the cerebral substance are also included, provided there are no permanent symptoms of concussion, compression or inflammation of the brain.

Article 1. *Of wounds properly so called.* Punctured wounds with pain and irritation are to be dilated, says our author, and then treated by compression. We conceive this direction much too general. Punctured wounds as such do not demand dilatation, and this operation should not be performed without a specific reason; as, the removal of foreign bodies, the securing of arteries, the liberation of inflamed tissues from fascia, &c. &c. Otherwise the usual treatment for wounds by adhesive strips and moderate pressure, and by preventing inordinate inflammation, is alone requisite.

Dr Gama is a strong advocate for the modern doctrines of adhesion as first promulgated by the Hunterian school in England, and he extends them to all wounds of the brain, penetrating or not penetrating, after the foreign bodies and splinters have been removed, and the hemorrhage arrested. While every surgeon of science and experience must acknowledge the great advantages of this practice in penetrating wounds of the head, yet many hesitate as to its general propriety in such accidents, as being sometimes productive of dangerous symptoms, especially where the brain itself is wounded. Our author notices two objections which have been advanced against

the immediate closure of severe and extensive wounds of the brain and its membranes: as first, the danger of hemorrhage occurring, and not being detected if the wound be accurately closed, whence result confinement of the blood, compression, irritation, &c. But there is no necessity, says our author, of closing the wound until the hemorrhage has been arrested, as it will then not be too late for union to occur; and there is no reason to expect a consecutive hemorrhage.

The second objection, that pus will be deposited within the cranium, is equally untenable. The idea that suppuration is necessary for the healing of cerebral wounds, can no longer be maintained; for facts abundantly prove that each tissue involved is capable of direct adhesion, not excepting the cerebral substance and even the bone; in this last it often occurs even without exfoliation. The oozing of sero-sanguineous fluid is preparatory to adhesion, and therefore beneficial; and there is probably no danger of this fluid ever collecting in such quantities as to be injurious; as owing to the proximity of the fluid to the surface, the separation of the fragments of bone, and the fixed condition of the brain, it will naturally escape by a sort of infiltration. Absorption also occurs very rapidly even in the cerebral substance, and even should inflammation and abscess result, these should not be attributed to the attempt at immediate union; for reunion, as will be hereafter shown, will have a tendency to prevent encephalitis. It can also be affirmed, that even if union has occurred in the skin, the effused fluid will present itself under the skin, and cause an ulceration of the cicatrix for its discharge; and on the other hand, should an injurious collection even occur, nothing would be easier than its immediate evacuation by the division of the cicatrix.

While approving of the practice here recommended, we would insist on great attention being paid to the discharge of this sero-sanguineous fluid, even when collected in but small quantities; as by keeping the edges of the wound separate, it retards or prevents immediate union, increases the liability to suppuration, and may induce symptoms of compressed brain; and although its usual tendency is external, yet large quantities may flow into the arachnoid cavity before adhesion occurs between the opposing surfaces of the arachnoid mem-

brane, as covering the dura and pia maters. Let the adhesive strips be lightly applied, the dressings be simple lint or ointment, and all pressure be most carefully avoided.

Article 2. *Fractures of the cranium.* Some observations are made as to the manner of their occurrence, and as to the important fact, that they may occur without any concussion of the brain, or any internal injury. The diagnosis of simple fractures is not always easy, and is of so little importance that no extraordinary measures for its detection are justifiable, especially as they may thus convert a simple into a compound fracture. In compound fractures, foreign bodies, if accessible by instruments, should be abstracted; loose or movable fragments are to be removed as much as possible, especially when driven into the brain or causing symptoms of compression; and the hemorrhage arrested. These being accomplished, "it remains to facilitate the immediate union of the soft parts, whatever may be their condition, or whether there be any loss of substance or not in the bones which they should cover. There is no necessity for interposing a wedge of wood or any other substance between the lips of the wound to facilitate the discharge of fluids, as recommended by Giraud of the Hotel Dieu, and by the authors of the article *Trepan* in the Dictionnaire des Sciences Medicales."

Article 3. *Operation of trephining.* Our author records a severe philippic against the use of the trephine, being a genuine disciple in this respect of Dessault, to whom the credit is due of greatly restricting the employment of this instrument, and of exposing the unscientific and really horrible practice of his contemporaries and predecessors. As many may, at this time, lightly resort to this operation, we will give the following summary of its dangers from our author. "To expose to the atmosphere an inflamed organ; to place the same agent in contact with a sound surface by which inflammation will be excited; to evacuate the product of diseased action without removing the cause; to accomplish no benefit in cases of extensive and deep seated effusions; to be of no service in cases of trifling collections of fluid which will be readily absorbed if the organization be restored to a healthy state; not to facilitate exfoliation in cases of fracture, and to retard it when in progress; are some of the objections which may be urged

against the trephine." P. 51. M. Gama also observes that prudent practitioners will almost in every case reject this operation, even in those cases in which it has been urged as a dernier resource. He argues, that when the cerebral affection resists the usual modes of treatment, it is no reason for confiding the cure of it to chance—to what is termed the last remedy. No one can fix the limits of our rational modes of treatment, especially in diseases of the brain, where wonderful recoveries have occurred when there was very little hope. The anti-phlogistic course proves advantageous, not only in relieving the inflammation, but also in facilitating the absorption of fluids.

Although granting the truth of most of these propositions, and condemning the frequent use of the trephine as much more injurious than useful, yet we cannot regard the operation as more deleterious than the formation of internal abscesses, or believe that the removal of the pus in such cases will be improper. It seems that our author does not believe that the collection of pus, by its pressure, aggravates the inflammation, resists the influence of remedial measures, or continually increasing in quantity, augments the ulcerative inflammation of the cerebral mass, while the symptoms of compressed brain are thus caused, and often aggravated to a fatal result. There appear to us much stronger reasons for opening such abscesses, than those which collect in any other tissue: and we should therefore say, that when antiphlogistics do not answer, when the patient, with the usual symptoms of inflammation, suppuration and compression of the brain, does not improve, the operation should be performed, and that it thus constitutes, not an irrational, but decidedly the most rational plan of treatment. Of this, more hereafter, when on inflammations of the brain.

Chapter II. *Wounds injuring the membranes and the brain.* These, when most severe, demand the same treatment as those already noticed; but in many cases, other consequences result, which do not characterize simple wounds, and which are to be recognized chiefly by sympathetic affections. These consequences are known by the name of concussions or commotions of the brain. Of these, there are various degrees; they are only to be regarded as a pathological state when there is stupor, or other affection of the vital powers, which remains for

some time. Concussions may occur directly from blows on the head, or indirectly from falls on other portions of the body.

Article 1. *Direct concussion.* The symptoms of concussion we need not transcribe; they are accurately detailed by our author, who believes that they are indicative of a depression (un affaiblissement) of the powers of the brain. "In some cases, there is a sudden abolition of all nervous influence; vital power is destroyed, and death occurs instantaneously." Much has been said of the state of the brain in concussion. Perhaps a large number of existing pathologists are still so mechanical in their notions, as to admit of no concussion, or indeed of any disorder, without rupture of tissue or some other structural derangement. We have, however, long believed and taught, that in concussion there is necessarily no rupture; that the symptoms are altogether independent on any solution of continuity or on effusion upon the cerebral mass, being indicative simply of loss of power, temporarily or permanently. We shall translate some observations of Gama on this point, who, we are pleased to find, advocates similar sentiments.

"Many examples are cited of dreadful concussions, accompanied by the sudden destruction of the principle of life. From these, we may refer to what occurs in milder cases, where the actions of the organs, which are merely suspended, and their functions apparently annihilated, soon resume their energy and regularity, and where it will be difficult not to admit, that the concussion, carried still farther, would have destroyed completely in the organic tissues the power of reaction, without any necessity, in order to explain the cause of death, to suppose a lesion, appreciable or material in the structure of the brain. The contrary opinion is however generally admitted: many do not separate concussion from lesions of structure, which ought, say they, to accompany it; they cannot conceive that it should be otherwise. There can be no doubt, that these frequently co-exist, but is this always the case? Do we not observe various causes producing sudden death, without any physical derangement of the very complicated (deliées) fibres of the organ of perception? There are some passions which cause cerebral depression, analogous to that which results from a violent concussion: as excessive fear, a sudden unexpected joy, &c. All know how carefully we must conceal danger, or announce news which will excite lively emotions, when individuals are sensible and nervous. Exciting passions, such as anger, cause in their development a commotion (ebranlement) very remarkable; all strong impressions,

indeed, suddenly made, are felt in the brain before it reacts on the injured organ, and can, by the disturbance they excite, annihilate its powers. It is evidently, by the commotion felt at once in the brain and in all parts of the individual, but principally in the nervous system, that lightning, or a powerful electric discharge, causes death with so much rapidity. Although lightning has various destructive effects on the human body, it is no less true, that it often leaves no trace of its course; that it kills as a subtle poison; and that absolutely it does not in any respect alter the structure of the tissues. Commotion has alone operated in these cases, as Franklin himself imagined. Finally, when a body of large size but small weight kills by falling upon the top of the head, is it not to the complete destruction of the vital power that this effect is generally owing?" P. 66.

We may add that actual minute dissection, by experienced anatomists, has not revealed any lesion of structure in many cases of death from concussion. The author alludes to some interesting points relating to this subject, in the form of questions, to which answers cannot be satisfactorily given in the present state of our knowledge, although any pathologist might present his own explanation.

"Concussion no doubt affects particular portions of the brain more than others, and in the same manner. If this affection be without alteration of structure, would there be partial death in the brain?

"Would the part, thus deprived of the power of receiving impressions and of transmitting sentiment, then enjoy more than a vegetable life? would it remain in this state in the midst of those which had preserved their privileges?

"Is it to this loss of vital power in a circumscribed part of the brain, that we must attribute certain partial and permanent paralyses?

"Is it to the same cause that the alteration in certain intellectual functions should be referred, when the senses and movements are unaffected?

"Have certain diseases, as epilepsy, which are manifested after a fall, the same origin?

"Will it accord with observation and facts, that ruptures of the brain should be present to explain these phenomena, especially when concussion has been followed by no other kind of accident?"

Article 2. *Indirect concussion of the brain.* This arises from falls or blows acting through any portion of the body except the cranium itself, and does not essentially differ from

direct concussion. The author has made some experiments (by means of an artificial brain, formed of a glass matrass filled with a solution of isinglass of about the same consistence as the brain) to determine the manner in which blows, whether direct or indirect, affect the brain.

Indirect concussion, operating through the spine, can never produce a separation of the dura mater at the point opposed, viz: at the summit of the head, nor occasion, when the force is of the same intensity, an affection of the brain as powerful as when the percussion is direct. It is hence more frequently exempt from complication, and reduced to a state in accordance with the strict acceptation of the word, concussion.

Article 3. *Concussion with organic lesions of the brain.* Solutions of continuity in the brain or its membranes, which may complicate concussions, do not necessarily involve any alteration in the vital power or influence of the nervous mass. Contusions and fractures often exist without concussion, and heal readily. Ruptures of the fibres of the brain may occur, from the slightest degree of contusion to disorganization more or less complete; these last of course being followed by death, usually suddenly. Partial or circumscribed disorganization may exist without any immediate bad symptoms, and may sometimes be followed by recovery, as has not unfrequently been observed from gun-shot wounds of the head, where the ball has passed with great velocity. Concussion has been almost nothing in such cases. In all solutions of continuity, there must be some hemorrhage; this is often trifling and much diffused; when in large quantities bad effects result, which will be hereafter noticed.

Wounds of nerves and of the brain unite by adhesion—cicatrices are formed, and the integrity of the functions is fully restored; and "it is a practical remark of importance," says our author, "that contusions and moderate ruptures of the brain are not only susceptible of cure, but during this process the functions of the brain are often undisturbed. This organ, notwithstanding its injury, influences, as usual, all parts under its empire; it supports its normal relations with the organic nerves; and the fibrillary reactions of the (sentient) surfaces irradiate towards the brain with equal regularity; indeed life exists in all its plenitude, and individuals believe themselves

in perfect health." P. 84. But usually, inflammation is inordinate, and injurious consequences from effusion, suppuration, ulceration, &c. follow, sooner or later, impairing or destroying the functions of the cerebral mass.

Chapter III. *Paralyses which follow wounds of the head.* "The impossibility of voluntary motions results from lesions of the spinal marrow, or the brain; but since it is the parts below the place of injury which suffer from the interruption of the nervous influence, we must rise to the brain as the only source of motion. The contrary opinion will be in opposition to the well demonstrated principle of the unity of action in the nervous system. The inferior portions of the system are merely the organs of transmission of the cerebral volitions. The communications of the splanchnic ganglions with this apparatus serve to draw from the common source the elements of nervous influence, without which even the great sympathetic nerve would be inert. In a word, there can exist no vital movement without the agency of the brain among those beings which are provided with this organ, any more than the circulation can be continued without a heart, or without that which supplies its place in certain species of animals." Such are the general sentiments of our author as to the influence and importance of the brain acting on all the organized portions of our system, by means of its various dependencies. This subject he follows into minute and valuable details, endeavouring to illustrate the physiology by minute attention to the phenomena and consequences of disease, as well as by the more common methods of observation and experiments on living and healthy animals: illustrating physiology by pathology; perhaps the best means for its successful cultivation. We shall endeavour succinctly to present his opinions, but we cannot examine his arguments for their support, or his criticisms on other physiologists, both of which deserve attention.

He believes in the *localization* of cerebral faculties, and hence that alterations of a determinate portion of the brain will paralyse those organs dependent on it, and whose nerves proceed from the point of lesion. Little however is known of the mechanism of these cerebral faculties, or of their mode of influence.

We are several times cautioned not to confound true paraly-

sis with the impossibility of exciting motion as arising from other causes; for example, individuals in a state of coma are insensible to impressions, and of course manifest no motion in their voluntary muscles. This is not a paralysis, but merely a quiescent condition from torpor of the brain.

The author believes that little or no dependence can be placed on vivisection in illustrating the phenomena of the nervous system, from the great disorder which necessarily results, from the difficulty of completely isolating any individual organ, from the influence of exposure to air, &c. in these operations. He disbelieves the present generally received opinion of the existence of particular nerves for sensation, for motion, and respiration. "The nervous action which constitutes sensibility, cannot, in my opinion, be separated from that which presides over motion; one cannot act without the other; or rather both are identical. The various shades of sensation or perception, and those, not less varied, of motion, are uniformly confounded in the several organs. Cabanis has observed that every impression which is perceived produces motion. No line of demarcation exists between the mechanical or contractile movements and those which are inseparable from sensation: nature passes from one to the other by insensible gradations. There exists in our organization only one vital principle, from which arises sensibility, and everywhere the actions of life consist in the effects of sensibility, accompanied by motions more or less extended, rapid, and capable of impressing the senses of the observer. We may therefore conclude, that all paralyses are lesions of the same nervous property." P. 90.

Of course our author does not leave unnoticed the works of Mr C. Bell, or of M. Magendie and their coadjutors; but he criticises them, particularly the former, very severely; points out the inconsistencies, and even the contradictions of which they are guilty, and certainly, taking into view the whole train of his observations and facts, has gone far to invalidate their opinions, or at least to render it desirable that new experiments and facts should be brought to bear on this most intricate and important subject. He quotes M. Lallemand (p. 274 of the 11th letter of his anatomical and pathological researches on the brain) as the physiologist who has best explained the

double property of sensibility and motility of the nervous system, and as having happily resolved the problem of incomplete paralyses, viz: the persistence of sensibility when there is loss of motion. "Always," he says, "when paralysis interests only the muscular system, it is feeble; which is tantamount to saying, that sensibility continues longer than myotility. But voluntary motion of a limb is the result of a spontaneous action of the brain. The perception of the impression produced at the extremity of a nerve, is an act independent on the will, which consequently does not demand that the brain should spontaneously act. It can easily be imagined that a portion of a diseased brain may be sufficiently altered as not to exert an active influence over the nerves dependent on it, and yet not so much so as not to receive impressions collected by the same nerves. What proves the truth of this explanation is, that patients may sometimes be observed, who, enjoying sensibility, but not capability of moving voluntarily a paralytic limb, nevertheless retract it, when the skin has been severely pinched."

Sect. 1. *Sudden paralysis from rupture of the brain.* The sudden and momentary insensibility and immobility from certain concussions of the brain do not deserve the name of paralysis, which gives an inadequate idea of the lesions of organs that take place. But paralysis may be of short duration, and may result sometimes from cerebral ruptures, and then the alterations of nervous texture have a perfect analogy with those which occur at the end of certain apoplectic attacks. Our author examines sudden paralyses as they affect the intelligence, the organs of sensation, or of voluntary motion.

Article 1. *Loss or perversion of intellectual functions.* M. Gama adopts the outline of Gall's doctrines of phrenology; the mental powers have for their special organ the anterior portions of the cerebral hemispheres; but these lobes have a marked influence on the functions supposed to be dependent on the posterior lobes of the brain. The intellectual functions therefore result from cerebral operations more or less complicated, of which the variety depends chiefly on our impressions. The divisions and subdivisions of the cranioscopist are too arbitrary; we are born, not with a number of cerebral organs,

completely formed, but with certain aptitudes, moral, industrious, scientific, &c., all having a more particular tendency to the anterior parts of the brain. The different faculties of mind, as well as the individual organic aptitudes, can be weakened or destroyed in consequence of cerebral injuries. Thus memory is sometimes destroyed while the judgment and the will remain sound: and sometimes the judgment is abolished while the will and memory remain. These and other singularities M. Gama would seem to refer, not simply to the wound of the brain, but to the concussion of the nervous mass; neither would he use the term, *paralysis*, as applied to them, as motion, voluntary and involuntary, is still continued. Intelligence may be weakened, disordered, or rendered incomplete by the loss of one or more of its faculties for a variable time, but a paralysis of the organ would cause all the vital phenomena to cease instantaneously, and death would result.

Article 2. *Loss, or marked diminution of the sensitive functions.* Paralysis of the organs of sense is comparatively unfrequent, even in cases where the muscles of the apparatus are affected; the taste, for example, is seldom impaired, should there even be a paralysis of the muscles of the tongue; hearing and sight are more frequently involved. Hearing not unfrequently is injured from a concussion of the internal ear; while vision is chiefly affected by cerebral concussions and congestions.

Article 3. *Paralysis of voluntary movements.* There can be no combined or regulated motion without the agency of the brain; this organ is necessarily the regulator as well as the source of all combined movements. The mechanism by which this is effected is not understood; it can be merely observed that the nerves are the agents, the inferior divisions of which are lost in the muscles, while their superior extremities, entering the brain, terminate at the seat of volition, the intellectual centre, which they assist in forming conjointly with the nerves of sense. Thus the same organ presides over the intellectual faculties, the sensorial perceptions, and voluntary motions.

It has not been found that the cerebellum, when most injured, produces any paralysis in the limbs, or that it is usually more concerned in our exterior movements than in the mani-

festation of our intellectual faculties. It would seem to preside chiefly over our involuntary actions, and the unity of action in the nervous system is preserved by intermediate nervous cords, establishing an intimate communication between those parts which are subjected to or removed from the influence of the will. The eighth pair is the most important of these nerves, and a lesion of that portion of the brain to which it corresponds, by paralysing the respiratory movement, which is connected with all others, would cause speedy death. This indivisibility of the nervous system can alone explain the vital phenomena in health or disease.

Paralysis may arise, therefore, from injury of the brain, as well as of the intermediate nervous cords between this organ and the muscles concerned: and hence M. Flourens is incorrect in supposing, that the brain being injured, the principle of motion would continue to exist in the tubercula quadrigemina, in the medulla oblongata, or in the medulla spinalis, as these are mere agents for the sentient and perceiving extremities of the nerves. Injuries of one cerebral hemisphere are followed by a paralysis on the opposite side: but there is no reason for believing that the fibres from the cerebellum decussate each other. Many observers have remarked that the movements of respiration have suffered at the same time with those of the limbs, but they have not hence deduced the consequence of the identity of the source of motion for both. But respiration often preserves all its energy in hemiplegic patients, as other muscles supply the functions of those which are paralysed; the muscles of the shoulder for example often act when the intercostals of the same side are inactive. "We have remarked also a defect of innervation in one portion of the circulatory system; this weakness does not proceed from the heart, since the arterial pulsations on the opposite side have an almost febrile excitement."

Sect. 2. *Paralysis caused by inflammation of the brain* The author in the preceding section does not state very explicitly how paralyses result suddenly from injuries of the brain, but seems to believe that they are dependent on concussion, and not on the mere fact of rupture of the brain. He however seems inclined to the opinion that in most cases there must be some actual derangement of structure in a por-

tion of the brain, which is the essential cause of paralysis; that simple pressure, or even concussion, can seldom be deemed sufficient. He now observes that inflammation of the brain, however obscure or chronic, may, by altering one or more portions of this organ, give rise to paralysis, and proposes to examine on what organ the cerebral affection manifests its effects—and what are the nervous agents whose functions it destroys.

Article 1. *Injury or loss of the intellectual functions.* The loss of the intellectual faculties can only be partial, if complete existence would cease. Of these, the will exists the longest, and if this should fail, from the inflammatory congestion, the instinctive movements would not remain after the movements of relation are destroyed; hence death results. The brain therefore is essential to organic life.

During inflammation of the brain there is delirium, or a sudden exaltation or perversion of one or more intellectual functions, not referrible however to an affection of any one portion of the hemispheres, as each of these affections can exist alone, while the brain preserves the exercise of the remainder of its functions.

The exercise of speech is enumerated among the intellectual actions, and this is often suspended by cerebral inflammation. Can this be owing to a paralysis of the tongue? Our author thinks not: because the complicated apparatus for the exercise of speech is not simply confined to this function, but is concerned in mastication, deglutition, the exercise of taste, of respiration, &c.: and we cannot conceive that there could be a paralysis of the apparatus without involving these other functions. In truth, the exercise of speech demands, 1, the intellectual act; 2, the formation of sound; and 3, the muscular movements for articulation. If one of these be absent, speech is lost. If there be no ideas formed, or no voice from injury or paralysis of the larynx, the integrity of the muscular apparatus of the face and tongue will be of no avail—the patient will be silent. Hence the loss of speech is not the result of a partial paralysis, but almost always an important phenomenon, indicative of the nature and seat of cerebral affections.

Article 2. *Consecutive loss of the sensitive functions;* 1, *of*

their nerves in the interior of the cranium, and 2, *of the anterior part of the brain.* In the organs of sense, we observe the proper nerve of perception, and also the apparatus destined to convey to it, and to modify at the same time, the materials for its exercise. In the eye there is the optic nerve, and also the apparatus of the globe and its appendages, which are furnished with other nerves by which the various portions are most intimately associated. A striking distinction should be made between the nerves of sense and those of the apparatus for sight, hearing and smelling—the functions of the former depend however on the integrity of the latter, and are capable of being rendered active or feeble, as the nerves of the apparatus are excited or depressed.

A similar distinction cannot however be made for the function of taste. Anatomists have in vain hunted for a gustatory nerve; some have thus regarded the lingual branch; others the hyperglossal; even the glosso-pharyngeal has been suggested, but none imagine that it can exclusively enjoy this prerogative. There can be, says our author, no gustatory nerve, as none of the various surfaces of the mouth can exercise this function alone, unconnected with the others. The most savoury body cannot be tasted when placed simply on the tongue, or on the palate; but gustation results from the combined action of opposing surfaces. Taste is not confined to the tongue; of course no nerve of this organ can be exclusively a gustatory nerve.

In investigating the functions of the senses, much has been accomplished of late years by experiment and observation;—the vital accessory actions have been elucidated, but the degree of their importance has not been established. M. Magendie has even maintained, as is well known, that perception is not reserved exclusively to the proper nerves of sense, but that the fifth pair contributes directly in transmitting to the brain the sensorial impressions. Our author devotes much space to the detail of cases and observations whereon to prove that the fifth pair of nerves, however important as an accessory nerve to the tissues involved in the sensual apparatus, cannot be a substitute for the peculiar nerves of sense, as the olfactory, optic and auditory: and also to show that the functions of this trigeminal nerve and those of the portea dura are identi-

cal; that neither is exclusively a nerve of feeling nor of motion, but that both are nerves for sensation and motion. The details are very interesting and valuable, and throw much light on this intricate and important subject of nervous agency. P. 192.

M. Gama also endeavours to prove, p. 203, "that the seat of the perceptions, that of sight as well as of the other sensorial functions, exists in the anterior part of the brain."—And, p. 212, he observes that "the accidents resulting from a percussion of the cranium are often attributed to a commotion which the eye has sustained, which are however the consequence of a concussion of the brain."

Article 3. *Paralysis of voluntary motion from cerebral inflammation.* Cerebral inflammations from wounds pass through different stages before there is an actual loss of motion in the muscles. Spasmodic muscular contractions, convulsions, subsultus tendinum, precede paralysis, and announce the progress and intensity of the cerebral affection. The disorganization from inflammation, which induces paralysis, comes on gradually, and during its stages, the irritated brain stimulates the muscles and excites them with violence. With few exceptions, it is usually impossible to ascertain the location of the injured portion of the brain in cases of paralysis, which would not be the case if the functions of the different portions of the brain were independent on each other, and perfectly known. The nervous mass enclosed within the cranium forms a system, of which each part contributes by its action to the functions of the whole. Experience and reason indicate that in those parts, folded (replicés) on themselves, contracted towards the base, the anterior and posterior have destinations which are not exactly the same. But ought we to imagine with Willis, that the cerebellum presides exclusively over the interior functions? Our author answers this negatively. He says that no part is independent of another; that the cerebrum and cerebellum co-operate in their influence on other organs. The cerebrum, which modifies the action of the viscera, alone directs the voluntary movements; and paralyses, attributed to the cerebellum, have had for their origin some concomitant cerebral alterations: and a great number of observations, worthy of confidence, prove that considerable disorganizations of

the cerebellum have caused disorders more or less marked of the internal functions, disorders which are reproduced in cerebral diseases not followed by paralysis.

Our author also seems opposed to the phrenological doctrine that the two hemispheres of the brain, for any other functions than the muscular action, are capable of acting for each other reciprocally, and an affection of one may, in certain cases, be unknown, the other continuing to act. He maintains that in all the voluntary and sensorial functions, the two hemispheres are so intimately connected that they must act simultaneously. The duplicity of exterior organs exhibits nothing else, relatively to their cerebral relations, than two radii departing from the same centre. Hence the puncture of one nerve of an extremity will often excite general convulsions.

Chapter IV. *Compression in general.* Compression arises from ruptures or contusions of the brain or its membranes, and also from inflammation, giving rise to effusions, and also to the production of solid bodies encroaching on the cerebral mass. Our author undertakes to demonstrate that paralysis is never the result of compression alone; a sentiment very different from that of most practical writers.

Article 1. *Sanguineous effusions.* The diagnosis of these is acknowledged to be very vague: for the injury of the brain is followed instantaneously by symptoms of concussion, and effusion usually occurs before these have disappeared, the symptoms of which can hardly be distinguished by the most experienced from those of concussion. Even where there is an interval of intelligence between concussion and compression, and effusion may be suspected, yet some other change may have occurred in the brain after the disappearance of concussion, giving rise to phenomena similar to those from effusion; as, for example, a sanguine congestion from the cerebral injury. Experiments on animals have not established the common opinion; for mere compression, where the texture of the brain was not injured, did not produce paralysis; when this effect resulted there was lesion of the cerebral mass.

Clinical observations confirm these views, for every surgeon is acquainted with cases in which no bad consequences immediate or secondary have resulted from compression suddenly or slowly induced, when the brain is uninjured. Cerebral irri-

tation is the real cause of the paralysis attributed to effusion; and it is certain that sensation and motion are never destroyed so long as the blood remains wholly in the vessels. There may result from this state an oppression of the nervous mass, a suspension more or less prolonged of voluntary actions and perceptions; but these are at once relieved by free sanguine evacuations. Neither will any interruption of cerebral influences be caused by a congestion of blood in the vessels of the membranes of the brain: meningeal apoplexy, so called, has really no existence. There must be a cerebral irritation, as the only cause of all these accidents. The author endeavours to support these opinions by a statement of cases, and commentaries thereon, although with much plausibility, yet we think not satisfactorily. Our readers must refer to the work itself for any argumentative details.

Article 2. *Purulent effusions.* Much pressure is exercised by pus deposited on the surface, in the ventricles, or in the substance of the brain, and this is often accompanied by deleterious consequences, which have been referred to compression. But numerous cases can be easily adduced, says our author, to show that most extensive collections have occurred without any notable disorder in the cerebral functions.

Should a patient, some time after a fall on the head, have somnolency, dulness, a sort of torpor in the extremities, these may arise from an abscess; but when in addition there are headach, coma with a hot forehead, inaction of the sensitive organs and loss of muscular contractions, we have indications, not of increased pressure, but of cerebral inflammation.

Article 3. *Formation of cysts.* These are observed in all parts of the body as the the result of inflammation, and of course in the brain also. They are analogous to the mucous membranes, and are capable of inhaling and absorbing. Abscesses are sometimes discussed, when the opposing surfaces of the cyst approach and unite, forming a linear cicatrix. Sanguine effusions often become enclosed with a cyst, by which the fluid may be absorbed. These cysts rarely become in any way injurious. They neither cause nor increase the paralysis.

Article 4. *Morbid solid substances,* which are not the result of inflammations of the cerebral mass, or originally developed in this substance, but arise from the dura mater or

other tissues, often cause great depressions in the brain. The compression however which results, is not followed by any injury to the functions of the brain, it is inoffensive to the nervous system, and will not produce paralysis.

We have no comment of importance to make on the doctrine here advocated—but acknowledge we are not fully of the author's opinion on the subject of compression. We regret that he has not more clearly indicated the evils from compression, however induced, as sometimes he alludes to pressure as never injurious, and again as causing stupidity, somnolency, imperfection in the muscular movements, &c. Facts can alone decide this question, and we considered it actually proved, that under certain circumstances, (what is not certainly known,) pressure will cause paralysis; but if the facts adduced by M. Gama be perfectly correct, the opposite sentiment may (however difficult to believe) nevertheless be true. Our own experience is not sufficient to form a judgment on this matter.

Chapter V. *Cerebral morbid sympathies.* In this chapter the author endeavours to point out the sympathetic derangements which result in the various organs of the body from injuries of the head, having before confined himself to those phenomena which involve the brain and its prolongations. He assumes it as a fundamental fact, "that all the actions of the economy, not excepting those of nutrition, have their principle in the nervous system;" but he also maintains that the nervous apparatus cannot maintain its own life without a supply of arterial blood; and, of course, that the circulatory system would soon fail without a supply of fresh materials from the digestive organs, and these last are also dependent on the nervous and vascular systems. These three organs, the brain, heart and stomach, "form a vital tripod, on which as a pivot roll all the organic actions; the study of the laws which regulate them serves as a basis for all physiology." He also regards the nervous system, not only as containing the principle of life, but as the source of the sympathies, the origin of all the actions of the economy. These views he amplifies in tracing the effects of injuries of the head on the *digestive*, the *genito-urinary*, the *respiratory*, the *circulatory*, and *cutaneous apparatus*, and illustrates the relations which the

nervous system maintains with all the organs of the economy. Hence he is again induced to examine particularly the uses of the nerves, and again encounters the opinions of Mr C. Bell respecting the respiratory and other nerves, which he examines with much critical detail and acumen. The whole chapter is exceedingly valuable to the physiologist, and of course is beneficial to the pathologist and practitioner; we must pass it over with slight notice, merely adverting to some few of his opinions.

He states here explicitly, that during concussion of the brain, this organ is in a state of torpor, which torpor or *ab-irritation* extends sympathetically to other organs. At the moment of the accident, there is a spasmodic affection of the stomach and lower bowels, by which their contents are evacuated; but soon these organs, and also all the digestive apparatus, fall in a state of torpor proportioned to that of the brain. These states would seem to demand stimuli; but these are useless in the first instance from the insensibility of the organs, and injurious subsequently by augmenting the reaction of the brain. In all cases reaction in the brain is followed by excitement in the abdominal viscera; each organ of the digestive apparatus is excited, not by its fellow, but simultaneously with every other. This excitement often terminates in inflammation; hence gastritis, hepatitis, &c., so commonly observed after blows on the head. Of the liver particularly he says "that it experiences, immediately after concussion of the brain, a state of stupor analogous to that which renders the intestinal canal immovable. It is then exposed, like the brain, to passive congestions; and when reaction follows, inflammation may be developed in parts surcharged with stagnant liquids, which have already irritated them, with as much facility as it is developed in the gastro-intestinal tissues." This hepatic inflammation does not arise, therefore, from the spreading of inflammation along the mucous tissue of the ducts, from the duodenum, as has been imagined; it has been observed, when no inflammation existed in the lining membrane of the ductus choledochus.

In speaking of the genito-urinary organs, he observes that there is often a spasmodic action or concussion, by which the urine and semen are involuntarily evacuated; but this does not

return; the organs are torpid. He combats the idea that the cerebellum is the only excitant of the genital system, especially during health; desire or will is requisite, which depends on the cerebrum.

Respiration, after the first moments of tumult, is slow, incomplete and irregular, unless death should be threatened, when it becomes greatly accelerated, as if all the remaining energies of the system were concentrated in the respiratory apparatus. M. Gama also observes "that encephalitis never determines a sympathetic irritation in the pulmonary tissue, or in the mucous membrane of the bronchiæ. The brain seems to communicate with this apparatus by means of the nerves, merely to support the respiratory motions. These, always homogeneous, have nothing morbid but their irregularity." P. 282. This irregularity, by interrupting hematosis, &c., may give rise to disease, as dyspnœa, suffocation, &c., of which a case is detailed. In this case the author believes that the sudden death resulted from the cessation of motion in the bronchiæ, and that hence the nerves of this tissue are the principal agents of respiration. He believes, however, that this agency is not exclusively confined to the pneumo-gastric nerves, but that "the ganglionic nerves excite the lungs in a manner differing but little from that of the eighth pair; and that they can, in certain cases, even alone, support respiration." Adult dogs, in whom the eighth pair on both sides has been excised, have survived for weeks, and some have completely recovered, the respiration being difficult only for a few hours or days after the operation. In all of them the blood was found stagnant, and as if combined with the parenchyma at the inferior part of the lungs, showing that the par-vagum influences the circulation of the blood. In the opinion of our author, Legallois first demonstrated that this asphyxia from the division of these nerves arose, not from the interruption of their influence on the lungs, but from a paralysis and consequent occlusion of the glottis, and hence by opening the larynx, respiration again became regular. Legallois did not draw from these facts the important inference that the motion of the lungs was in such instances supported by the branches of the great sympathetic.

The author enters into a severe and valuable criticism of

Mr C. Bell's views of dilatation of the thorax, lungs, &c., which ought to be consulted by those who investigate the physiology or pathology of the pulmonary apparatus.

As regards the sympathetic disturbance of the circulatory system, the author maintains, with most modern pathologists, that it is less when the inflammation is confined to the cerebral mass than when it exists on the surface of the brain or its membranes. He endeavours to afford a physiological explanation of this fact, for which we must refer to his work, stating merely that the heart and its vessels are not so directly under the influence of the nerves as other organs, although the connexion between the vascular and nervous systems is nevertheless intimate and essential. Hence the brain cannot cease to act without causing death, but its co-operation with the organic movements may be very feeble without danger; and our existence is less jeopardized from cerebral injuries than if the movements of the heart had been exclusively submitted to the same nervous action. P. 316.

The cutaneous tissue is sometimes excessively sensible, after injuries of the head, which is frequently a very unfavourable symptom; but our author considers it as indicative of spinitis rather than of encephalitis.

Chapter VI. *Inflammation of the brain and its dependencies.* This inflammation is termed *encephalitis;* under which term should be included the inflammation of the whole or part of the cerebral mass, and also those of its immediate envelopes, the pia mater and the arachnoid membrane. *Meningitis* should include only the phlogosis of the dura mater.

The traumatic affections of the brain, as well as of other viscera, should be always regarded under two divisions, those which arise from the direct effect of the injury on the organ, and those which result secondarily from the morbid organic movements in the same tissues. These last will of course produce numerous alterations in the appearance, consistence, &c., of the cerebral mass; but nevertheless they are all the result of irritation and inflammation, and therefore should be regarded as the same affection, and not be sub-divided according to the various states in which the affected part may be found. Unfortunately the relation between the symptoms and the direct and indirect lesions of the brain has hitherto been

very imperfectly pointed out, and hence much remains for subsequent inquirers, who should be careful not to render the subject still more obscure by fanciful suppositions or subtle divisions.

Owing to its intimate and vascular connexion with the brain, in all its extent, the pia mater, says M. Gama, is constantly irritated at the same time as the substance of this viscus, whether the irritation originated in the brain or in this tissue. From the pia mater it is slowly transmitted to the arachnoid. Our author now enters into a long, but interesting and important investigation respecting the susceptibilities of the arachnoid and other serous membranes, and the frequency of their diseased actions. He argues from the anatomical structure, the absence of red vessels, and the want of sensibility in their normal state, that they are seldom originally affected, and inflammation in them is excited by the subjacent tissues, and this always gradually. This view, he thinks, is confirmed by a consideration of the exciting causes of cerebral inflammations, whether contusions, changes of temperature, the exciting or depressing passions, and also by an examination of the symptoms of supposed arachnitis, as given by the most approved authors, and of the appearances post mortem. To Lallemand, professor at Montpellier, he gives much praise for his able investigations of injuries of the brain, and points out the decided valuable contributions which he has made to our knowledge on this subject; but he condemns as erroneous his views of arachnitis, and by criticising several cases, so termed, from Lallemand's letters, he endeavours to prove that the symptoms supposed to indicate inflammation of this membrane, are in reality those which necessarily result from inflammation of the brain, and from it alone. Hence he infers that it is impossible to separate in practice these affections, and the attempt to effect this only renders the subject more obscure. We have long believed that there was much that was fanciful in the supposed frequency and importance of arachnitis, and have maintained that inflammation of this membrane, if it really existed in many cases, was accompanied by irritation of the brain, and was hence of but secondary importance. To this we were led by the symptoms during life, and by the appearances post mortem. We think, however,

that M. Gama has gone much too far in denying an original inflammation to the arachnoid and other serous membranes, and in undervaluing the importance and the strength of such disease, however induced. Who has not met with the most intense pleuritis or peritonitis, where there has not been any evidence of inflammation of subjacent tissues? and it is certainly a gratuitous assertion to say that such inflammation existed originally, and had been discussed; we can hardly suppose that inflammation of the pleura would persevere to a fatal result, while the original affection of the contiguous cellular membrane, lung, or intercostal muscles, existing at the same time and in the same system, would be resolved. There can be no doubt that, owing to peculiar anatomical and physiological circumstances, arachnitis is more rarely an independent affection than pleuritis, the irritation readily passing from and to the contiguous tissues, but its occasional original occurrence from contusions, from insolation, cold, &c., and its permanency without involving the brain, can hardly be denied on sufficient foundation. The assertion therefore of M. Ribes, as quoted by M. Gama, seems to us absurd, that the serous membranes are analogous to the cuticle, and incapable of inflammation; it deserves not serious refutation, and but weakens confidence in other opinions which our author has advanced.

There is another circumstance in encephalitis of importance, and we are happy to find it noticed by M. Gama; it is the combination of inflammation, "excessive organic action," with a vital depression of the affected tissues: or, in the language of Hunter, for it is truly his idea, it is increased action with diminished power. This, says our author, is often seen in every part of the body, and is always unfavourable—extensive suppuration or mortification often resulting. In concussions of the brain, this depression occurs; "the brain is plunged into a state of debility and stupor," and when reaction occurs, the symptoms are complicated and less intense, compounded of those of irritation and depression; the diagnosis uncertain and the treatment difficult. P. 367.

"It is to analogous causes we must attribute the softening of the brain (ramollissement), sometimes increased to absolute fluidity (diffluence), in subjects who have exhibited but obscure symptoms of encephalitis, and who, for this reason,

have appeared to some observers to enclose in their cranium a destructive principle *sui generis*. In these individual cases, the cerebral inflammation excites no reaction; it is marked by a vital depression, similar to that which succeeds concussion." P. 368.

Articles 1 and 2 are occupied in detailing in a concise and perspicuous manner, the *symptoms of acute and chronic inflammation*, and of the *alterations which occur in the external parts during the progress of the inflammation.*

Speaking of these alterations in the scalp, the author refers them to irritation arising from the organic relations established between the parts exterior and interior to the cranium. "The carotids form within and without a capillary network, the affections of which are often identical, because it arises in both situations from the same trunk, and constitutes a common system." This anatomical argument has had, we believe, an improper influence in these cerebral affections. It may be true, when irritation is excessive, that the interior and exterior inflammations may co-exist with equal intensity, but these, experience has abundantly shown, are exceptions to the general rule, that inflammation exists with difficulty at the same time upon the internal and external surfaces of the cavities of the body. After injuries of the scalp and brain, the former soon inflames, suppurates, and even granulates, before any cerebral disturbance is manifest. When encephalitis is established, then the inflammatory symptoms of the scalp, instead of becoming aggravated, diminish; the granulations disappear, the pus becomes sanious, and the wound returns nearly to its original condition; thus contradicting not only the argument of our author, but also that of those physiologists who attribute the circulation of the blood entirely to the heart. M. Gama himself recites cases with which most are familiar, in which phlogosis of the scalp has alternated with that of the brain and its membranes.

The author is not more happy in explaining the frequent occurrence of erysipelas from wounds of the scalp; for he asks, after alluding to the closeness of the tissues of the cranium, if a severe inflammation be here excited, whether the tumefaction which supervenes ought not to gain in surface that space which it cannot acquire in depth? We, who regard erysipelas as a specific inflammation, the peculiarities of which depend on derangement of the chylopoietic viscera, would account for its

frequency after cerebral injuries to the disturbance of the digestive organs, so commonly observed after such accidents, and to which our author has already referred. Vide p. 280 of this analysis.

Article 3. *Terminations of encephalitis. Resolution* is the first mentioned, and implies the absorption and return into the general circulation of the materials which the irritation had determined to the part effected. It includes the consolidation and cicatrization of wounds, and is facilitated by counter-irritations, accidently or artificially excited. Except in cases of wounds, adhesions very rarely occur between the dura mater and the arachnoid. "It would appear to be inconvenient, and that the continual motion of the brain resists their formation."

Serous and purulent exhalations are often found, according to M. Gama, on the surface of the brain, exterior to the arachnoid, or under the arachnoid in the delicate cellular tissue,* between the arachnoid and piamater, or between the convolutions of the brain; sometimes in the ventricles of the brain. These effusions should be distinguished from abscesses arising from wounds and contusions.

In severe cases, the whole brain is found covered with purulent matters: but these are not produced exclusively, says our author, by the membranes; for the latter are so changed as sometimes hardly to be distinguished, while the substance of the brain is softened, and even infiltrated with pus. The effusions are sometimes so abundant and yet so diffused, as to cover every portion of the brain and spinal marrow; in exemplification of which a case is detailed. Another case is given at page 482, to show that life is occasionally preserved by an abundant discharge of sero-purulent fluid, through the meatus auditorius externus. How such discharge occurs, is, we think, a moot point: our author cuts the knot by asserting that "a part of the humour insinuates itself into the canals of the petrous portion, and there excites a phlogosis of the inter-auricular tissues, resulting in their ulceration and the perforation of the membrana tympani, as happens frequently from severe otitis terminating in suppuration." P. 406.

Degenerations are all the result of irritation, and should in-

* Is the piamater any thing more than cellular tissue, supporting blood-vessels? Our observations would confirm the negative.

clude not only the generation of new bodies, but also the softening and other alterations of tissue. Their effects have been already noticed. A post mortem examination of a subject, dead of chronic encephalitis, exhibited the cerebral envelops of an unusual whiteness; the brain was diminished in size, and never could have filled the cavity which contained it; its substance was firm, hard, crackling under the knife; and the fibres, on attempting to rupture them, afforded as much resistance as the muscles when force is applied to them. The spinal marrow was equally indurated, and all the nerves seemed to be preternaturally solid. P. 416.

Chapter VII. *Treatment of traumatic injuries of the brain, and of their complications.*

Article 1 regards the *local treatment of wounds of the head.*

Article 2 embraces the *treatment of concussion.* The usual practice of the present day is decidedly recommended, of resorting to external stimulants to the skin and nose, and of avoiding depletion before reaction ensues. Sometimes sudden vesications by boiling water or by the aqua ammoniæ may be requisite. When reaction occurs, depletions are recommended, but here two rocks are equally to be avoided, that of taking from the brain too great a quantity of its natural stimulus, and that of leaving this organ too much congested. We must endeavour to balance the reciprocal influence of the heart on the brain, and of the brain on the heart; and also remember that the symptoms of cerebral inertia or torpor are often complicated with those of irritation.

Article 3. *Treatment of encephalitis.* This embraces debilitants and revulsives. M. Gama insists on general bleeding, especially in idiopathic inflammations of the brain; in those from wounds, it is less frequently necessary: but the most useful part of the treatment consists in local evacuations by leeches. He dwells on this point, and insists that as the disease is originally local, such remedies must be the most important. Experience must decide, independently of such reasoning, and we believe that testimony is at present, and will be, in favour of general bleeding, from which will be obtained, very generally, the good advantages of local depletion, and for which this last cannot be safely substituted. He thinks cups are too irritating, and that in some cases of wounds of the cranium, with encephalitis, the sinuses of the brain may be opened.

Theoretical notions of the modus operandi of *cold*, rather than careful observation, induce our author to restrict the employment of this most powerful sedative to moderate inflammations, or to the commencement or decline of the disease. He is afraid of the symptoms being *masked* by its influence, while disorganization continues; that leeches on the torpid skin can remove but little blood, and thus the most powerful means of cure be inefficacious. He is afraid also of the reaction from the use of cold, and hence recommends that its use should be gradually discontinued; but even then, he dreads the return and aggravation of the symptoms, and in conclusion affirms, "that cold is never indispensable; it is but rarely useful, and serves, thus to speak, only to stifle the symptoms of encephalitis, or to lose precious time in which the disease might be combated in a manner more direct and certain." To us it seems strange that such fears have not been completely dispelled by experience, but we suspect that dread of such imaginary evils has deprived our author of the advantage of experience. No precious time is lost by the use of cold in cerebral inflammations; if the symptoms be diminished or destroyed, so is the inflammation; disorganization does not continue after the symptoms have disappeared, and the impression of cold, affecting the internal tissues as well as the external, is important and almost indispensable in the most acute forms of inflammations, as well as in those which are more moderate. Neither is there danger from the reactions after the use of cold, and although no one would abandon sanguine evacuations because cold is employed, yet by this powerful adjuvant, the loss of blood will be greatly diminished.

Revulsives are much employed, and according to the representations of our author, are often injurious. *Purgatives*, so frequently recommended and relied on, are never to be given until the symptoms of general and local irritation are greatly lessened. M. Gama says they are useless in all cases of torpor of the bowels from cerebral disease; the brain must first resume its functions, then the bowels will act spontaneously; and often they are injurious, giving rise to putrid, malignant, and other fevers, complicating the original affection. "It is thus," says he, "that false theories not only conduce to an erroneous practice, but prevent the discovery of the evils which

they produce." With all deference to M. Gama, and to the school of which he is a member, we think that few *practitioners* exist, who will not apply this observation to himself and his coadjutors, and denying the supposed deleterious influences of cathartics, continue their use even in acute encephalitis. M. Gama thinks them useful in chronic affections, resulting from acute disease of the brain, and in paralysis and other affections from continued cerebral disease, he would also employ certain internal stimuli in moderate doses, so as not to irritate the mucous surfaces: such as the strychnine, so favourably recommended by M. Fouquier.

Stimuli to the skin can be useful only when the brain can be rendered, by direct antiphlogistics, sensible to their influence. They should always be regarded under the double relation of the pain which they excite, and of the afflux of fluids which they produce to the irritated part. Pain is always injurious in cerebral irritations; hence those revulsives are to be preferred which excite only moderate pain, and act on broad surfaces, so as to gain, by the extent of the parts stimulated, that which is lost in the power of the stimulus. Poultices, cataplasms, fomentations, or frictions with water and vinegar warmed, are often to be preferred to sinapisms, blisters, issues, and setons, not only as less dangerous, but as positively more beneficial.

Articles 4, 5 and 6, are occupied with *the treatment of sympathetic irritations*, with the *dietetic regimen proper during encephalitis*, and with the *attentions requisite during convalescence.* We notice nothing peculiar, except as respects the diet of patients with inflammation of the brain; our author's remarks on this subject deserve attention. "The diet ought to be severe as long as the symptoms maintain their violence, but when resolution is taking place, too absolute an abstinence from food will be injurious, and experienced practitioners will be careful of not carrying it to extremes. They know that the stomach, too long deprived of its natural stimuli, when the want of them is felt, becomes irritated, and that its suffering can, in cases of encephalitis, be felt by the brain and renew its disease. Would it not in reality expose patients, who have just been cured, or who have been threatened with a cerebral irritation, and whose digestive organs are sound, to the accidents produced by the deprivation of aliments, if they were

restrained to a diet too severe? As much as the dangers of a premature nutrition should be dreaded, so much are those from the opposite excess to be avoided. The degree of nourishment may be augmented gradually, as the first attempts indicate its good effects. It is moreover to be remarked, that aliments then act in the same manner as revulsives; that they produce a gastro-intestinal irritation, favorable to the disappearance of cerebral accidents, and that it is not unfrequent to observe the torpor of the intestines dissipated under their influence, after having resisted medicinal agents. The cerebral inflammations, when uncomplicated with gastro-enteritis, do not seem capable of being managed, like those of the digestive apparatus, by a prolonged and very severe diet. Wine and all stimulating fluids are positively interdicted, because they excite too readily the sanguine system and the nervous apparatus." P. 449.

Art. IX.—M'CULLOCH ON REMITTENT AND INTERMITTENT DISEASES.

An Essay on the Remittent and Intermittent Diseases, including, generically, Marsh Fever and Neuralgia: comprising, under the former, various anomalies, obscurities, and consequences; and, under a new systematic view of the latter, treating of Tic Douloureux, Sciatica, Headach, Ophthalmia, Toothach, Palsy and many other modes and consequences of this generic disease. By John M'Culloch, M.D., F.R.S., &c. &c., *Physician in ordinary to his Royal Highness Prince Leopold of Saxe Cobourg. Philadelphia. Carey & Lea,* 1830. *(Reprint from the London edition.)*

If ever we wished for a secretary, it was while transcribing this awful title page of Dr M'Culloch's essay. What with his "including," and "comprising," and "coming under," and "treating of," and "modes and consequences," we feared we should never get through his "anomalies" and "obscurities."

One thing, however, the reader soon discovers; the title page is characteristic of the work. We have seldom seen such meshes and tissues of words, involving such a paucity of ideas; so large a net to hold so few fishes. We had thought that Jeremy Bentham was the only writer of the present day who had the privilege of being at times incomprehensible, and generally obscure, in his codifying and proposed reforms of law. But it seems that we must make another exception in favour of the author of the work before us. And why should he not be entitled to the distinction? Medicine may lay claim to immunities as well as law, for her favourite legislator. There is, however, this little difference between the two cases of obscurantism; that, while Bentham is really a powerful and original thinker and a bold innovator—a true reformer, M'Culloch is merely a somewhat voluminous and unquestionably tedious writer, whose parenthetical explanations separate premises from inferences with as fatal certainty as the dread Styx was interposed between Orpheus and his Euridyce. We should think it unnecessary to introduce the Doctor to our readers were it not for his continued assumption of novelty of view, his affectation of Baconian philosophy, and his scepticism of the records of medical science, contradicted as they are by his poverty of facts, his limited experience, and strained deductions. He forgets that an ambitious flight without the eagle eye will only serve while enlarging the horizon to render all objects beneath indistinct, and that what is gained in extent is lost in accuracy.

The leading and to him cardinal idea, which he takes so much pains to inculcate as original with himself, is not, we think, one which Dr M'Culloch can really lay claim to on this score. When he asserts himself to be the first, and even now almost the only physician to regard tic douloureux and various forms of rheumatic and paralytic disease as connected with intermittent fever, in their nature and causes, he surely overlooks the literature of his profession, or has singularly limited knowledge of the opinions of his contemporaries. Was it left for Dr M'Culloch to tell us that hemicrania, that toothach, that racking pains of the face, that ophthalmia, that sundry aches of the limbs, assumed at times a distinctly periodical type, and are not only produced by the commonly admitted causes of

marsh fever, but actually alternate with the more common and regular paroxysms of this latter. If such be novelties of his exclusive promulgation, we have been labouring under singular delusion since we began some twenty years ago our medical studies; for nearly ever since that time we have considered these propositions as familiar truths to the profession. How, otherwise than on the long duration and general admission of this creed, can we explain the free use of bark and arsenic, and what were conceived to be kindred medicines in these anomalous products of marsh air.

If success attend Dr M'Culloch's claim in this case, and the medical public are so voracious as to swallow implicitly all his *olla podrada*, the temptation for many of us to imitate him will be irresistible. We shall next hear it gravely set forth, as novel doctrine, pregnant with grand practical results, that inflammation of the brain, and occasionally mania and idiocy, with certain sympathetic disturbances of the stomach and liver, are directly deducible from sun stroke, and that even on occasions hemiplegia and pains in the limbs may at times acknowledge a similar origin. What a prolific age of medical discovery shall we then live in, when every familiar proposition in medicine comes to be accredited as a recent discovery. There is but one thing required to realize this brilliant prospect: it is a burning of the three hundred thousand volumes on medicine, which, like that of the famed Alexandrian library, should leave us once more free to experiment and theorize, without being accused either of plagiarism in stealing others' knowledge without avowing it, or of ignorance, in assuming as our own what we ought to have known originated with, and belonged of right to others.

Some excuse may be found for Dr M'Culloch's inflicting such a weighty book on his professional brethren in Britain, if their opinion be really such as he describes it, when he says, "in every season and almost in every place in our own country, to this very hour, the autumnal and marsh fever is called a typhus, and considered a contagious disease." Such crude notions as these surely require correction, and yet we greatly doubt whether the creed of the author himself be any great advancement on the road of truth. When speaking of the length of time that is said sometimes to elapse between expo-

sure to malaria and the appearance of periodical fever, and of twenty days having been noticed in some cases as intervening between exposure and attack, he tells us that these cases still leave room for doubt; "*from our knowledge of the fact, noticed elsewhere, that a remittent will become, or perhaps produce, in any given individual, a contagious typhus, under confinement.*" Whether it be the remittent, or the individual, or contagious typhus to which the doctor refers, "under confinement," we cannot take upon ourselves positively to say: but the idea which he would convey by any of these suppositions is not one which will obtain currency among us. These passages occur in his first chapter, or that "on the ordinary remittent or marsh fever," and they may be regarded as among the chief novelties in this chapter. The author talks familiarly about "the production of paralytic affections by remittents." What writer of practical knowledge among us would advance such a proposition? Dr M'C.'s affirmation that marsh fever does act directly itself, or its generating poison on the nervous system, is not of remarkably recent origin; nor do we feel more convinced by his *proof* in "the state of apoplexy or profound coma with which the attack is sometimes ushered in." His is a shrewd conjecture, that the larger paralytic affections, such as hemiplegia, or palsy of the leg or an arm, consequences arising from intermittent as well as remittent, are probably direct actions on the nervous system. We should like to know what system, if not the nervous, is directly affected in such cases. It has never fallen to our lot to see such terminations as of palsy after periodical fevers, and we really think that our opportunities of observation have been, at the least, equal to those of Dr M'Culloch. We have witnessed in some fatal cases, marked by inflammation of the brain, phenomena of this kind preceding death: but they were not the mere product of the poison of malaria. Notwithstanding his magnified views of this latter power of causing palsy, he is constrained to admit that cold will give rise to the same effect; and he adds, "it seems to me that the hemiplegia of labouring people, often attributed to drinking spirituous liquors, is, sometimes at least, and probably very generally, the consequence of sleeping on damp ground, particularly after fatigue."

In the second chapter, "on the chronic or relapsing and

obscure or anomalous remittent," the author begins by offering some strictures on the divisions, made by Cullen, of typhus mitior, and synocha, and synochus. The first of these he regards as a modification of remittent, and probably as a fever of malaria; and as far as his own observations go, he should say that it never was a contagious disease, nor produced by contagion. Now if our readers will put this opinion in juxtaposition with the one advanced but a short time before by Dr M'C. "that a remittent will become, or perhaps produce, in any given individual, a contagious typhus, under confinement," they will have a tolerably fair specimen of the contradictions and obscurities in which he involves himself in so many parts of his work. We shall not attempt an analysis of it: the trouble would be poorly repaid by any good; nor can we, in charity, continue to criticise longer than to show the slight claims which such a work has on the confidence of American physicians, whatever may be its adventitious success on the other side of the Atlantic. It is high time for us to set up in these matters for ourselves, and not to take all the transcendentalism of Scotch and German metaphysical medicine for maxims by which we shall abide and be guided. We can produce much better things in this line at home, and it becomes those whose voices are heard as authority, to let this fact be known, so that enterprizing publishers may not be misled by foreign puffs to circulate among us what we do not want, and what is a real obstruction to us in the search after truth.

The following pleasant bull and truism open one of the author's paragraphs. "The *obscure* symptoms which I am now to point, are those, as might be anticipated, which whenever they become *marked* are also *easily discovered*." His remarks on muscular weakness as the most prominent complaint of some febrile patients, and on the anomalies of pulse, are common place, though shrouded in a tortuous phraseology. There is some originality in his idea that since the *primæ viæ* are affected in other disorders besides fever, they are therefore seldom of any value as diagnostic symptoms; but are rather consequences than causes. We know nothing of his personal history or of his opportunities for medical practice, and yet we do confess our suspicions that Dr M'Culloch utters mainly results of closet hypotheses, and by no means the inferences from enlarged and

diversified experience. We are strengthened in our suspicions when we find him alleging "that in the far greater number of cases occurring in our own country [Britain] the practice of blood-letting is most commonly nugatory or superfluous, or else injurious." Can we, in the remittent fever of the United States, consent to receive such a dogma as guide, in despite of the plainest experience, which leads to an opinion of a diametrically opposite cast from that just given. The author assumes, as a fact, that remittent fever is the product of malaria, and as such is not characterized by inflammatory action, and consequently, that it does not require the use of the lancet. So baseless is the fabric of this therapeutical superstructure! He thinks favourably of calomel in the chronic and relapsing variety of this fever, because biliary derangement is, he believes, the cause of the febrile return, and that calomel being the remedy for these, will cure or cut short the relapse. Is he sure, or will he now-a-days get physicians who have heard or actually witnessed the different phases of the calomel practice to agree as to its anti-bilious properties, and the necessity of continuing this medicine as recommended by the author so long as it produces the evacuation of morbid bile. The most juvenile practitioner in this country is now well aware that he can cause morbid bilious discharges whenever he chooses, merely by giving calomel. Emetics are the remedy first noticed by the author, and then purgatives. He is not partial to the first, and recommends of the latter those, such as the resinous, which shall produce a local effect. The use of wine and bleeding he treats of separately. It is thus that he examines the value of therapeutical agents in an abstract sense, without passing them in review, as they would naturally present themselves to the mind of the practitioner at the bed side of his patient. Of mercury as a remedy acting on the system at large, he is not disposed to think favourably in the fever under consideration. We transcribe the following paragraph, which we fear is yet but too applicable to a large number of the American physicians.

"To enumerate the evils now daily produced by this medicine in all its forms, in innumerable disorders and patients, by what a French writer, censuring English practice, calls '*l'inevitable calomelas*,' and not only in real disorders, but in imaginary ones, and in subjects that may be considered healthy,

would be a task worth undertaking; while it is one on which I must not venture in this place. It is, or has been recently a fashion, almost amounting to an insanity, and which has not merely abused this active and hazardous substance in medical hands, but which has rendered it familiar to those, a now numerous class in this age of universal aspiring and of assumption, especially on the subject of medicine, who take upon themselves to treat their own diseases, real or imaginary, and still less pardonably the ailments of their acquaintances. If, besides the unconscious dead there are thousands living to rue the day when mercury became the universal medicine in the hands of English physicians, there are tens of thousands who, if they were conscious of the fact, should lament the hour when first they imagined themselves qualified to practice that which he who has studied it most and longest finds the most difficult of arts, and the more he studies it the more difficult, and who should repent too that they ever knew even the very names of 'calomel and blue pill.' Well may entire Europe wonder at English practice; and more still may it express its surprise that universal self-empiricism, before which all the professed quackery of all Europe shrinks into nothing, and in comparison of which the temporizing *ptisanerie* of France is absolute philosophy."

When, in the subsequent paragraphs, Dr M'Culloch cautions against the free or hasty use of mercury in disorders of the "visceral glands," which are connected with relapses and chronic states of the remittent fever; and when he tells us that he has unquestionable evidence of the aggravation and protraction of these fevers from the use of mercury, we are disposed to join with him in the propriety of the caution. But he goes on to say that he has been inclined to doubt whether it should even be hazarded when there is a demonstration of visceral disease; adding the following strange assumption as an admitted fact: "and if, as is supposed by the most judicious physicians, the fever is itself the cause of the glandular affection, it is plain that the imaginary remedy becomes in these cases poison." Now it seems to us that the tendency of the belief among the most judicious physicians is the very reverse of what the author gives them credit for. It is thus that he indulges in a strain of vague generalities, couched in an obscure style, and then forsooth we must gratefully receive the whole lumber as an important addition to our existing knowledge and materials for new theories. Some attention to general and morbid anatomy, and to their connexion with physiological

and pathological phenomena, would have been better preparation for his task than the doubtful logic of Dr M'Culloch, by which, so far from enlightening, he actually obscures the subject. A *questio vexata*, after being argued by him, is not a whit fitter for judicial decision than before: it only becomes a little more involved, so as to force us to throw it into medical chancery, to become matter for definite judgment in the next half century. With all this blundering in pathology we see marked contradictions in his therapeutics. Here we find him reprobating mercury in visceral obstructions associated with remittent fever. A few pages back he praises calomel as the best remedy for a derangement of the biliary functions, so often met with in chronic relapses.—Briefly expressed, he tells us that mercury is generally pernicious in disorders of the visceral glands or remittent fever, but that it is useful in disorder of the liver in this same fever. This is one of the evils in which a diffuse writer so often involves himself.

The fifth chapter is on dysentery and the cholera. The opening sentence is fresh evidence of the exceedingly partial views which the author takes of the etiology of diseases, and of his proneness to assume as a fixed maxim what is still a disputed question. "Since dysentery," he tells us, "is one of the most important and severe of the disorders produced by malaria, I could not," &c. We deny this disease being, as a matter of course, the product of malaria. Jackson speaks of it as most prevalent in the interior of the West India islands, "*at an elevation of five or six hundred feet above the level of the sea, amongst a series of mountainous ridges not directly exposed to currents of exhalations from swamp and low grounds.*" In our own country dysentery has prevailed with more fatality and in wider range in the upper limestone regions, than in the low alluvial soils where intermittents and remittents are so common.

Intermittent fevers, obvious and obscure, take up a hundred and fifteen pages of the work before us. We cannot find in them adequate reasons for the author detaining us so long on this subject. If the common places, the obscure, and the conjectural were to be omitted, his three chapters on this form of fever would be singularly abridged and greatly improved.

Some details are entered into respecting the state of the patient's mind, as marked by irritability or feeling of despair, and occasional desire to commit suicide or murder, even when the common febrile symptoms of intermittent are indistinctly marked. "It is well known that the desire for suicide is often exceedingly temporary, even when it is most overpowering; and I remarked formerly, when treating of remittent, that if controlled with resolution, for even a short time, it will pass away."

But it is in the chapter on the anomalous, obscure and simulating intermittents, that the author riots at large in the mists of erroneous generalization. He triumphs at discoveries which he certainly ought never to pretend to have made, had he studied with a singleness of purpose the writings of his predecessors. Do not the *febres larvatæ of* Frank, the neurosthenic diseases of Giannini, and the *fievres intermittentes pernicieuses* of the French, present us proofs of the knowledge possessed by various authors of the anomalous intermittents, long before Dr M'Culloch undertook to teach us the institutes of fever. He refers to the apoplectic or comatose state, which sometimes ushers in an attack of intermittent; and is at great pains to tell us of "the danger of error as to such an attack," since recourse to blood-letting, "commonly resorted to by practitioners under such symptoms, be the cause what it may, is often to induce palsy, sometimes death, and in any case, to produce additional bad symptoms, often of a very serious nature." Our own complaint would be, that blood-letting is *not* commonly enough resorted to in such apoplectic cases by practitioners. We have ourselves had recourse to this means of relief by leeches in large number in one fit, and cups in the second, over the epigastrium, in both cases, and with marked benefit. Nor was the coma here at the beginning of a disease, but at the close of a violent and protracted attack of remittent fever, in which depletion had been freely practised. The quinia was afterwards administered, and the convalescence was rapid.

Dr M'Culloch talks of receiving from Italy English patients rendered paralytic under Italian practice. Such wholesale censure as this ought to be substantiated by the most positive evidence; and yet, though himself professing to be in possession of the whole history "most precise and clear," he does not

condescend to furnish his readers with their undeniable data. Was there nothing in the constitution and habits, and prior diseases, and obstinacy and duration of the fever of his English patients, at the time, that should explain their paralytic state? Nothing, absolutely nothing but the Italian practice? We have had, for two months past, a person under our charge who had been seized with what the author would call a malarious palsy of one side of the face. We had this person, Dr M'C.'s strong warnings to the contrary notwithstanding, bled and cupped at the early period of the disease, and with marked benefit. He is now nearly well; the deformity is gone, and voluntary movement of the muscles is almost entire. The fact is, that in all the neuralgic maladies of M'Culloch, or the same ones called neurosthenic by Giannini, bleeding is neither to be expressly prohibited, as it has been done by both those authors, nor as a matter of course resorted to without regard to the accompanying symptoms and the state preluding the attack. In the case to which we have just referred, we had recourse to the carbonate of iron and strychnia with advantage, and also a small permanent vessication just before the ear, on the cheek.

We need not follow the author in his enumeration of all his anomalies, since, however new to him, we really do not think them mysteries to the profession at large, whether they be maniacal ravings, abnormal palpitation and pain of the heart, rheumatic pains in other muscles, catarrh, obstructed menses, obstinate headachs, dyspeptic symptoms, &c.

When speaking of the remedies in intermittent fever, Dr M'Culloch's language respecting arsenic is any thing but safe for the young practitioner. He who is so pointed in his reprobation of the free use of mercury seems to entertain no fears from the liberal administration of a much more virulent agent. After enumerating the symptoms following the ingestion, in excess, of arsenic, but short of its properly poisonous effects, he adds that he has never seen ill consequences resulting, and very composedly tells us that "they are all easily removed by brandy, as is the common surfeit, or by opium, and if not, they cease of themselves in a few hours." An ingenious comparison that, *as is the common surfeit!* He gives us no warning against its prolonged use and often consequent de-

struction of digestion. He reprobates venesection in chronic intermittents,—the very form of this fever which we have, in a large, very large number of cases, most successfully treated by blood-letting. Intermittents of months and years duration, or which had been but partially suspended during that time, have been in our hands completely and permanently removed, with the effect of giving health and strength to the patient by the use first of the remedy which is thus pointedly forbidden by the author. Were the subject one of less vital importance, we should feel at times inclined to suppose he amused himself with mystifying his readers, as in the paragraph in which he speaks of wine or spirituous liquors, "used in the chronic diseases so as to produce intoxication." After showing on what principle this plan sometimes succeeds, and how it injures when it fails, as he admits to be most generally the case, he draws this singular conclusion: "Thus we can easily judge of the degree to which wine may be allowed as an article of diet, and of the restrictions to which a patient labouring under chronic intermittent ought to be subjected." If any reader can form this kind of judgment from the doctor's premises, we shall consider him as gifted with second sight, and able to see through a mill-stone. In the way of sage apothegm, it is only equalled by the opinion of Dr Wilson Phillip, in a late work, that wine is innocent, and allowable when it agrees with an invalid. We have not room to follow the author in his dietetic remarks on wine, and to reply to his strictures, on what he conceives the undue restraints attempted to be imposed on its votaries. With him, one grand morbid agency, malaria, is enough, and requires not the additional explanations which would be furnished by intemperance, &c. He speaks of the ascetic principle, which is instilled into us through our whole education. Our own opinion is, that though in early life we are often forbidden this or that article of luxurious diet, or means of sensual enjoyment, it is too often from whim or from selfishness on the part of the excluders, and as a punishment, but not from an ascetic principle. The prohibition only extends to particular junctures, but is not, as it ought to be, regulated by a fixed principle, which some with the author may call ascetic, but which we humbly conceive to merit more the designation of moral or religious.

There are some good remarks on fashionable and domestic quackery in the chapter on the cure of intermittents, which we regret not having room to extract. The author animadverts with some point on the universal tampering with salts and calomel, and a course of the waters at Cheltenham or Lemington, as a mode of passing time equivalent to any other expensive system of idleness on which society has stamped a certain reputation. Alluding to the habitual use of purgatives, he says, "let any family, or any individual thus educated on purgatives, (provided indeed that the health is not utterly ruined,) take but courage enough to destroy the medicine-chest and lock the door against the physician, and they will soon find which was the cause and which the consequence." But when the author tells us, that it is in the hot stage of an intermittent that purgatives are best administered, we feel disposed to believe that he speaks from hypothesis, not observation. General experience, sustained by very good theory, is adverse to such a recommendation. Purgatives, at this time, will distress an already irritable stomach, and besides being rejected, will often augment the fever and cerebral disorder.

As we have already remarked that we cannot see the authenticity of the claims to novelty and sound doctrines set up by the author, in his tedious disquisitions on neuralgia in general, we shall not attempt either to give an analysis of his opinions, nor, *seriatim*, citicise his unfounded pretensions. Perhaps, hereafter, when reviewing the work of Teale on Neuralgia, we may be induced to notice the concluding chapters of Dr M'Culloch's work. In the mean time, it is sufficient to say, that these chapters on *tic douloureux*, *periodical headach*, *various neuralgia in other parts of the body*, *sciatica*, *questionable neuralgia*, *neuralgia from injury of nerves*, *toothach*, *neuralgic ophthalmia*, exhibit a constant assumption of originality of view, illy borne out by the context; and what might be regarded as original, is so from the obscurity of dialectics and involved phraseology, rather than from any really marked novelty of idea. If Dr M'Culloch cannot find that physicians have regarded periodical headach and vertigo as a variety of intermittent fever, his researches have been certainly much more circumscribed than those of very many, we may say the majority, of the reading and observing part of the profession.

So, when he intimates that for toothach extraction of the offending tooth is universally recommended, even when it is not decayed, he must have very limited notions of the knowledge of his medical brethren, and very limited intercourse with them in the practice of their profession. Substitute rheumatic for neuralgic, and we believe in this case the one term as luminous as the other; and what physician, or even old woman, is not aware that in such kinds of toothach, extraction of the tooth will not prevent the one next to it from becoming the seat of pain, and so on along the entire course of the jaw. In neuralgic or rheumatic ophthalmia, even admitting, what we strongly doubt, the universally miasmatic origin of the disease, the practice recommended by the author is not a legitimate deduction from the premises, nor can we join him in reprobating evacuations, sanguineous and others. This carries us to his chapter on the cure of neuralgia. Dr M'Culloch reprobates the lavish and empirical use of the narcotics in the various affections included under the general term neuralgic. He regards tonics as the chief and most energetic remedies, and of these the most efficacious are bark and arsenic. Need he be told, that if any strong example of extensive empiricism were to be adduced, it would be the free use of these two medicines in all the varieties of intermittent disease, call it fever, or neuralgia, or neurosthenia. And then what a heedless, we may say mischievous spirit of generalization is that which he displays in taking bark as the type of all the astringents, aromatics and bitters; and arsenic as the type of the metallic remedies, usually had recourse to in paludal fevers and periodical disease. Happy for us, we are not under the necessity, at the present day, of taking refuge behind such conjectural philosophy as this. It was hardly worth while for Dr M'Culloch to take the trouble of writing a work of nearly five hundred closely printed pages, in order to tell the world, that remittent and intermittent fevers, dysentery, cholera, have common paludal or malarious origin; that an attack of one of these diseases is sometimes supported by local or neuralgic pain, which is also periodical in its recurrence; that all the neuralgiæ are of malarious origin, though he himself admits that some of the worst forms may be produced by cold; and finally, that the best remedies for these various diseases are bark and arsenic. If

he has assailed empiricism of one kind, and should succeed in overthrowing it, he merely substitutes another variety more extensive in its place, and one which we do not hesitate to say, will be more mischievous, than that against which he levelled his attacks.

His commentaries on the above simple text, new to him, but as we conceive, familiar enough to his readers, are very obscure as pieces of composition, and utterly disappoint the inquirer after new and practical truths. The whole of them might, with great advantage, be brought within the compass of a hundred pages, whether we regard the reputation of the author, or the good of the medical public who should feel inclined to become his readers. As it is, we cannot but regard the essay of Dr M'Culloch, with such a long title, as a work of vast pretensions and small merit, magnificent promise and feeble execution. An extract from his preface will serve to convey our ideas to the reader, if the latter will just substitute the words "the essay of Dr M'Culloch" for the word "physic." "The language of truth is simple and brief, but that is not the language of physic. Its words have meanings, and the same words have always the same meaning; but this is not the language of physic. The language of error is multitudinous, variable, vague and unsteady; and this is the language of physic. If there be a philosophical reader who doubts this, if there be a logician, a man accustomed to evidence, who has not read medical books, let him read even the most celebrated, and be satisfied." We would add, let him read the essay of Dr M'Culloch and his scepticism on this point cannot fail to be removed. Not even the ingenious idea of this author, to attempt the solution of a medical problem by algebraical formulæ, can save him from this judgment. The labours of the iatro-mathematical physiologists, such as Keil, Borelli and others, are good recorded evidence of the futility of attempts to represent, by fixed and absolute quantities, that which is hourly changing. Details of facts, phenomena which are facts, and inferences from them, are required in medical philosophy; but our process of reducing them to their true value is not by means of algebra and the mathematics. We fear that Dr M'Culloch has taken more pains to prepare himself in these latter branches of human knowledge than in pathology

and therapeutics, for the composition of his essay. As unprejudiced judges, we must, however, declare that we have read with much satisfaction many passages scattered through the work, in which he exposes, by argument and ridicule, the excessive proneness to the poly-pharmacy or ultra-drugging of his countrymen, and countrywomen also, for the mania is not confined to the members of the medical profession alone. We cannot but regard these passages as the best part of Dr M'Culloch's essay, which, in connexion with our opinions already expressed, is equivalent to saying, that he has utterly failed in his main design, and has only been fortunate in the incidental, or as it were, parenthetical points.

ART. X.—ANATOMY AND PHYSIOLOGY OF THE GANGLIONIC NERVOUS SYSTEM.

Recherches Experimentales sur les fonctions du Systeme nerveux ganglionaire, et sur leurs applications à la Pathologie. Par J. L. BRACHET, D.M. *Medecin de l'Hotel Dieu de Lyon, &c. Paris,* 1830. *Pp.* 447.

De Nervi Sympathetici Humani Fabrica, Usu et Morbis Commentatio Anatomico-Physiologico-Pathologica. Tabulis æneis et lithographicis illustrata. Auctore JOH. FRID. LOBSTEIN, *Medicinæ Clinices et Anatomiæ Nosocomi Civium Medico Obstetrico Primario; Plurium Societ. Medicorum Sodali. Parigium,* 1823, *4to, pp.* 174.

The physiology and pathology of the nervous system are subjects of avowed difficulty, and upon the investigation of which every writer blessed with a reasonable portion of modesty enters with hesitation and fear. All who have paid a due degree of attention to physiological inquiries, and kept pace with the progress of that science, must be fully aware, that while our knowledge relative to the mechanism of many of the functions of the animal economy has, in consequence of the discoveries made in anatomy, as well as of the improved state

of chemistry and natural philosophy—the results of vivisections and a more careful analysis of pathological phenomena, advanced, within less than a century, to a gratifying, we had almost said an unlooked for degree of maturity; we are indebted to recent investigations, nay, to our very contemporaries, for the few positive facts we possess in reference to the normal and abnormal conditions of the nervous system; very many points connected with this portion of our economy remain, however, to this day, and we lament to say are likely to remain for a long time to come, subjects of dispute among physiological inquirers. Let the reader consult the writings of Bichat, Reil, Rolando, Bell, Magendie, Bellingieri, and a thousand others we could mention, and he will be at once satisfied that there is hardly a single portion of the nervous system on the functions of which the opinions of the best writers in the science are fixed and unanimous—in which the most contradictory theories have not been advanced and stubbornly supported.

Aware of this circumstance, it cannot be regarded as surprizing, that an individual uninfluenced by a preconceived and favourite theory, in endeavouring to form for himself, or attempting to present to others, a just idea of the present state of our knowledge on the subject, should feel the difficulty of the task, and approach it with diffidence and an apprehension of failure. Notwithstanding this, however, with all the difficulty of the task staring us full in the face, we propose to devote a few pages on the present occasion to the consideration of the physiology of the ganglionic or sympathetic nerve, which of the various parts of the general tissue collectively designated by the term *nervous*, is the least understood, and hence requires to be investigated with the least possible delay.

To this undertaking we are led by a sense of the duty imposed on us as editors of a periodical work, of laying before our country readers, who, from the circumstances under which they are situated, are generally unable to refer to original sources, a concise account of the present state of opinion on the more important subjects connected with their profession. This of course is not always the most agreeable circumstance incidental to our editorial career. But we shall always gladly submit, if by so doing we can be useful to our patrons.

For thus *sacrificing* ourselves for their benefit or amusement, we hope we shall be entitled to a small share of gratitude on the part of the unlearned; while from the initiated we solicit a large fund of indulgence for the imperfect arrangement of the following pages, the substance of which we derive, not only from the works of Professor Lobstein and Dr Brachet, but from various other sources, as well as from our own personal observations and reflections.

The great sympathetic, so called from its supposed agency in establishing a relationship of action between the different parts of the body, has also been denominated by some, in consequence of its being distributed to the three great cavities of the body, *trisplanchnic*, while from others it has received the name of ganglionic, in consequence of its presenting a great variety of ganglia—a circumstance on which was founded a doctrine we shall presently have occasion to notice—and by others, from its connexion with the intercostal nerves, that of great intercostal.

This system of nerves, which we shall call indiscriminately by the first or third of these names, is composed, as we have just said, of a series of ganglia, connected to each other by means of intermediary filaments, and of a few longer and thicker cords. It extends from the basis of the cranium to the lower part of the trunk, and communicates by means of small filaments with the spinal nerves and with several of those that pass out from the cranium. It sends off, moreover, from its several ganglia, nervous branches, which accompany the arteries, and are distributed in a more particular manner to the organs of the involuntary functions. This nerve has been distinctly traced along the internal and external carotids, the basilic and part of the brachial arteries, as well as to a considerable distance along the femoral artery. Such, indeed, is the extent to which the filaments of this nerve have been traced along these vessels, and other larger trunks, and such is the difficulty experienced in explaining, without their concourse, some phenomena we shall have occasion to allude to, that the anatomists of the present day have generally thought themselves justified in concluding, that the ganglionic system furnishes filaments which accompany all the arteries in their ultimate distribution to every part of the body; and there are facts

which even lead to the belief, that, from the same source is derived the main supply of nervous power possessed by the capillaries.

At its superior part the sympathetic is concealed in the carotid canal and in the cavernous sinus, and assumes there the appearance of a gangliform plexus, sending off several small branches, two of which join the sixth encephalic pair, while a third communicates with the vidian branch of the fifth pair. By means of the same pair it communicates likewise with the ophthalmic ganglion, which, indeed, was considered by no less an authority than Bichat as belonging to, or in other words, as constituting a portion of the sympathetic. Should this view of the source of the ophthalmic ganglion be admitted as correct, it must follow that this ganglion is the superior of all those belonging to the order of nerves we are now considering. Beclard and other anatomists class the spheno-palatine and the maxillary among the ganglia of the sympathetic. When we trace the sympathetic after its escape from the carotid canal, we find it to pass downwards on each side of the spine, as low as the sacrum. In this course it presents many ganglions, which may be enumerated thus: three in the neck, which from their situation and respective positions are denominated superior, middle, and inferior *cervical;* twelve in the region of the back, and called *thoracic;* five in the lumbar and three or four in the sacral regions, which have received the names of *lumbar* and *sacral* ganglia. The sympathetic nerve of each side terminates at the coccix, either by means of a small ganglion denominated from its situation coccigian, or by uniting with the corresponding branch of the opposite side.

By most writers on the nervous system the ganglionic nerve has been considered as an unit; in other words, as *one* system, charged with certain functions, which, though differing in their results, belong to the same class. Other writers have divided it into two portions: one *central*, situated in the abdomen, over and around the large vessels and in the neighbourhood of the viscera; the other, *peripheral*, composed of ganglia situated in various parts, along the course and on each side of the spine. M. De Blainville also has drawn a line of demarcation between the more deeply situated ganglia, to which he assigns the duty of presiding over the

nutritive functions, and the more superficial, united together by filaments, and contributing to the formation of the great sympathetic cord, the function of which he considers to be that of establishing a communication between all parts of the organism. To the latter he in consequence applies exclusively the name of sympathetic, which has been given usually to the whole system. This view of the subject, founded on the anatomical difference existing between the two portions of the ganglionic system, on the diversity of the two sets of functions, which, as we shall see, are referred to this system, as well as on some circumstances attending its evolution, is highly plausible, and would merit, even had it not the sanction of a name so justly celebrated as that of M. De Blainville, the attention of every physiologist. However, in this article we shall treat of the ganglionic system as a whole, without any reference to the above division of it; the number of facts in our possession not appearing to us sufficiently numerous to justify any decision as to the precise portion of the mass of ganglia which presides over one or other of the sets of phenomena commonly referred to the sympathetic system generally.

The ganglia are bodies irregular in shape, though assuming in general a roundish and oval appearance. They are enveloped in a double coat of cellular membrane, and are supplied for purposes of nutrition with a sufficiency of blood-vessels.

As regards the intimate composition of these nervous enlargements, a difference of opinion exists among physiologists. Mr Brodie, in his lectures delivered before the College of Surgeons of London*, has given in the following language what he considers to be the sum of our knowledge respecting their structure.

"They appear," he remarks, "to be formed by an enlargement of the nervous substance, and are of a complicated structure. Into ganglia the nervous fibres may be traced, and from these ganglia the nervous fibres again emerge. Scarpa has paid much attention to the fabric of the ganglia, and he gives the following history of it. He says that the fasciculi of nervous filaments which enter a ganglion are sepa-

* These Lectures are yet unpublished. We derive the above extract from Dr Wilson Philip's paper on the functions of the nervous system, inserted in the volume of the Philosophical Transactions for 1829; and subsequently in Dr Philip's new work on acute and chronic diseases.

rated and divided from each other, and that they are combined anew. A nervous fasciculus entering a ganglion divides into smaller fasciculi. These divide again, and cross and intersect each other at various angles. Then the divided fasciculi become again united, and as at first, they divide into smaller and smaller fibres, so that when they begin to unite, they form gradually larger and larger bundles. At last the nerve which entered a ganglion emerges from it, with its fibres collected into one or more fasciculi. Sometimes several nerves enter a ganglion, in which case they are all blended together, forming a complicated network, in which it is impossible to determine what belongs to one nerve and what belongs to another. Every fasciculus or filament which enters a ganglion passes through it. There is no appearance of any one terminating in it.

"If," continues Mr Brodie, "we unravel the texture of a ganglion, we find that each nervous fibre retains its own peculiar neurilema; but, besides this, the spaces left between the intersection of the fibres are filled up with a peculiar soft substance of a grayish or yellowish colour."

We have given this extract from Dr Brodie's lecture because it contains a pretty fair summary of the opinion of Scarpa, whose work on the ganglia we have not been able to consult, and not from any intrinsic merit it exhibits.

Bichat, who followed the Pavian professor, after stating that the tissue of the ganglia has nothing in common with that of the brain and neuralimatic canals, which, according to him, should be classed among the fluids, remarks that the former does not appear to be fibrous; all signs of a linear or filamentous organization being completely unrecognisable on simple inspection. He considers this tissue as nearly homogenous in its nature, and affirms that when cut into slices it presents a uniform appearance. Bichat allows, however, that he has not repeated all the experiments of Scarpa, and refers to the work of his Italian predecessor, and to the plates it contains. He nevertheless thinks there is something else in ganglia than a mere resolution of the nerves into very delicate filaments. "A glance," he remarks, "enables us to discover a marked difference between the ganglia and the nerves,—a difference as great as that existing between the cerebral nerves and the brain itself; the colour, consistency, and other outward qualities, as well as properties, of the two being quite dissimilar." "Were the nerves that come from the spinal cord merely unravelled in

their passage through the ganglia, this would be but a difference of form, not of nature, and their properties, therefore, would remain the same. Why has not nature placed ganglia on the nerves of the limbs as on those of internal parts. If there be only a resolution of nerves into finer filaments in the ganglia, why is there no constant proportion between the fibres that enter on one side, and those that issue from the other? Did the nerve that penetrates the superior cervical ganglion from above, expand in its interior, and reunite its filaments from the cord that issues from below, it ought evidently to be precisely of the same size at its exit as at its entrance. Such a relation, indeed, between the nerves of the opposite extremities of ganglia ought to be general. But a very cursory examination shows that a contrary disposition almost invariably obtains. The size of the ganglia should be relative to that of the nerves whose expanded fibres are said to compose them. Why then are the intercostal ganglia so small, and the trunks which unite, or rather to use the common phrase, originate, and then leave them, so large? And why, on the contrary, is the superior cervical ganglion so voluminous, and its nervous branches so minute? How can the frequent interruptions among the ganglia in the human body, which in a host of animals are quite regular, be explained, if the nervous filaments which enter be continuous with those that issue from them?" It is no slight support to the opinion of Bichat, expressed in the above extract from his General Anatomy, to remark, that Sœmmering has made very similar reflections on the same subject. As regards the mucilaginous matter pointed out by Scarpa, and before him by Winslow, and which according to them is interposed between the nervous filaments of the ganglia, Bichat remarks, that it ought to predominate considerably, since the ganglia surpass much in volume the nerves which are supposed to give them origin. He adds that he has never seen this substance; that he does not know what it is, and that a ganglion when divided appears quite solid.

Since the time of Bichat, however, the majority of anatomists among his countrymen, as well as in Germany, Italy, and England, have admitted the fibrous nature of these bodies, as well as the existence of the peculiar additional substance we have mentioned.

According to Haase it is sufficient to cut a ganglion in order to see a number of small white points which are nothing more than the divided extremities of the filaments. Professor Lobstein remarks, and in this he is supported by Béclard and other anatomists, that the filaments which penetrate into or emanate from the ganglia, observe a straight direction when the ganglia are formed by a single nerve, while they interlace with each other whenever these bodies are formed by several nerves. He informs us, that in a nerve which had been allowed to macerate a long time, he found that the medullary cord which had emanated from the ganglion situated above it, passed through all the succeeding ganglia, and in doing so united with other cords, constituting the substance of the latter.

Mr De Blainville, who in consequence of his regarding with Gall, Reil and Walther, and against the opinion of the elder Walther and Sœmmering, the gray matter of the medulla spinalis, of the medulla oblongata, and of the peduncles of the cerebellum and brain, and other portions of the central masses in the light of ganglia, draws a distinction between these and the ganglia properly so called. To the former he has given the name of pulpous ganglia, while to the others, which include the ganglionic system of every writer, he applies the term *non pulpous* ganglia. The former he regards as composed principally of neurine, and doubts the existence in it of the fibrous structure on which so much has been said. The others he considers as constituted of nothing but an interlacement of fibres. In a word, he considers them in the light of nervous plexuses, in the same way as the lymphatic ganglia consist of plexuses of lymphatic vessels; and does not even allude to the pulpy matter of which we have spoken. When physiologists and anatomists of the standing of Scarpa and De Blainville differ so materially on this point, the existence or non-existence of a peculiar pulpous matter in the ganglia of the sympathetic, it is difficult for less authoritative writers, unless backed by an extensive series of experiments, to decide between them. Yet as Scarpa's opinion appears to have received the sanction of a large number of the most accurate investigators; and as De Blainville, on the other hand, so far at least as we can learn, stands alone in his opinion on the subject, and has not supported it by the results of chemical analysis or dissection,

we must, with due deference to him, lean on the opposite side of the question;—in a word, we go with the majority.

The question here presents itself, are the ganglia of the sympathetic similar in organization with those of the other nerves, or do they differ from these in a tangible manner? From the quotation we have presented from the lectures of Mr Brodie, it appears evident that this distinguished physiologist and surgeon, and consequently Scarpa, from whom all his information on the subject of the ganglia is derived, have considered these small nervous masses in a general way, and adopted the same views in reference to the structure of them all, whether belonging to one or other of the different orders of nerves. This, indeed, is a common occurrence among writers on this subject, particularly of the English and German schools. Yet these writers, while maintaining that, however different from each other the ganglia may appear in form and situation, their general structure is with unimportant differences the same in all, allow that the simple or spinal ganglia differ in several respects from the compound ganglia, by which name Scarpa designates the ganglia of the sympathetic system: 1. In the situation they respectively occupy; 2. In the kind of envelope with which they are invested; 3. In the mode in which the nervous filaments pass through them and from them. By Wutzer, who in his elaborate work on the subject of ganglia,—*De Corporis Humani Gangliorum Fabrica, &c.* has furnished us with one of the most satisfactory accounts of the anatomy of these nervous bodies, and who considers the gasserian, the ciliary, and the maxillary ganglia as belonging to a different order from the simple and compound, and as *cerebral;* three sets of these bodies have been made; 1, those we have just mentioned, (the cerebral); 2, those belonging to the spinal system, and 3, the vegetative, or those connected with the organs of involuntary motion.

The following account of the differences existing between these various sets of ganglia in respect to envelope and structive, is given in the language of Mr Craigie.* "Void of the dense strong coat with which the others are invested, the

* General and Morbid Anatomy, p. 269.

cerebral ganglions consist of soft secondary matter, connected to the filaments of one, or at most of two branches, and are arranged with less complexity." (Wutzer.)

"The spinal ganglions are said to possess two coverings, one of which resembles the cerebral membrane, *(meninx dura)*, the other the soft cerebral membrane, *(meninx tenuis, pia mater)*. The non-spinal, or compound ganglions, have also two coverings, which are merely different modifications of filamentous tissue less dense and compact than in the former." "In the simple ganglions, the filaments of which they consist invariably follow the axis of the ganglion; but in the compound ones they are found to rise towards the side and emerge from them; and upon this variety in the direction and course of these filaments depends the variety of figure for which these two orders of ganglions are remarkable. These nervous threads *(stamina s. fila nervea)* described by Scarpa, correspond to the medullary filaments *(fila medullaria)* of Wutzer. According to this anatomist these filaments, when about to enter the ganglion, lay aside their neurilem; yet they are sufficiently tough to resist a certain degree of tension."

Besides these, other differences may be added; the great variability and irregularity in shape of the non-spinal ganglia and their connexion with several nervous trunks or branches. The pulpous substance in them is so strongly united to the medullary fibres, that it is very difficult to separate them from each other—circumstances which do not obtain in the cerebro-spinal ganglia. Independently of this, it may be stated that this pulpous substance appears to differ from that of the other set of ganglia, in being harder, of closer texture, and greater tenuity. This is particularly observable in the cœliac ganglia and in those of their plexus.* M. De Blainville is a supporter, and certainly none abler could be desired, of the idea of a difference between the spinal ganglia, which according to him are pulpous and partake of the nature of the nervous centres and their cords, and the true ganglia which, as we have seen, he considers to be non-pulpous. We need hardly remark that the force of his remarks on the subject will rest solely on the degree of accuracy that may be conceded as to the anatomical difference between the two sets of ganglia.

But whether these points of dissemblance should be consi-

* Beclard's General Anatomy, § 805, 806.

dered as sufficient only to establish the existence of a slight *variety* in the nature of the two sets of ganglia; or whether they would justify us in regarding the compound ganglia as differing completely from the others, and thus lead us to infer a difference of function, is a point on which we will not pretend to hazard a decided opinion at present, and to which we shall have occasion to recur in a subsequent part of this article.

The peculiar substance of the ganglia of which we have so often spoken has been considered by many anatomists as analogous to the cineritious substance of the brain; but Scarpa regarded it as consisting of soft cellular substance filled with a peculiar matter differing in various individuals—being mucilaginous in emaciated subjects and yellowish and oily in those that are fat. It will be sufficient to remark on this subject that the experiments of Bichat, and subsequently of Wutzer, show that the matter of the ganglions is not acted upon by chemical agents, in the same way as is the gray substance of the brain; and that contrary to the opinion of Scarpa,—an opinion subsequently adopted by Meckel,—Bichat, Wutzer and Béclard, they conclude from their experiments and researches, that the ganglions are never converted into fatty matter. Béclard merely admits that in very corpulent subjects, a fatty substance accumulates within the membranous covering of the ganglion, and hence surrounds the latter. But this is very different from the idea of Scarpa and Meckel.

The nerves which by their assemblage compose the sympathetic are of three kinds: 1. Those which establish the communication between the ganglia themselves and serve to convert the whole into a single system or apparatus; 2. Those which unite the ganglia with the thirty pair of spinal nerves; and finally, those which are sent off from the ganglia to the various parts of the body. The first and second set consist of short and white filaments, resembling somewhat those of the spinal nerves. The third set, which is the most important of the three, appears to be composed of filaments of a different order. According to Bichat they are of a reddish colour, soft, delicate and small, and unlike the former, indivisible into filaments or capable of being separated from the neurilima. It is right to state, however, that Scarpa, whose observations have been confirm-

ed by Lobstein, holds a different sentiment on this subject from Bichat, and while admitting a few external differences between them and the other nerves, considers that *all* the branches of the sympathetic are similarly organised and composed of small filaments which interlace with each other in the form of plexuses. The various filaments of the third set of ganglionic nerves differ in external appearance in various parts of the body; some are thin, delicate, and rather flat than cylindrical; others are thick, soft and almost transparent; others long, fine and white, &c. Uniting with the ganglia, and not with the intervening nervous cord, they entwine the arteries, accompany them to their minutest ramifications, and may hence be considered as one of the generating elements of the organs. In many parts these nerves form, before accompanying the arteries, inextricable plexuses, to which particular names have been given, and from which the branches arise that follow the arteries.

It appears, therefore, from the preceding remarks, that the nerves, which by their assemblage constitute the sympathetic system, differ not only among themselves, but from the cerebro-spinal filaments. Besides the differences we have noted, it may be added that the nerves of the sympathetic do not present the same symmetry of arrangement, regularity of distribution, and constancy in the number of their filaments, as are observed in the nerves of animal life. To these differences Bichat adds the following: 1. The organic nerves possess a greater degree of tenuity at the place of origin, in or at their escape from the ganglia—the filaments breaking more easily at any other place than at that point; which is the reverse of what occurs in the nerves of animal life. 2. Sometimes the ganglion is prolonged for a short distance under the form of a cord. This is particularly the case with the superior cervical, the lumbar, the semilunar, &c. 3. The nerve arises abruptly from the ganglion in the same way as a muscle attached to a bone. This is readily seen by dividing the superior cervical ganglion, and the nerve which escapes from it, downwards. 4. The dense cellular envelope of the ganglion is continued on the nerve, which by this means acquires a greater degree of consistency. When this envelope is carefully removed each fibrile is seen to arise separately from the ganglion. In some places each filament remains distinct; in others, they unite

together in form of a cord. M. De Blainville, who, as we have seen, entertains different views respecting the organizations of the ganglia, affirms that the nerves of organic life present *all* the characteristic signs of the non-pulpous ganglia—the same semi-transparency, the same rosy or grayish hue. He indeed states in the most positive terms, (vol. ii. p. 412,) that the difference between the two sets of nerves is considerable, and that the nerves become more and more modified in their aspect, in proportion as they are removed from under the influence of the will. The ganglionic nerves, he continues, are more difficult to separate into filaments than those of animal life; an operation, indeed, which cannot be effected without the aid of maceration. M. De Blainville further remarks, that no *sensible* difference can be detected between the nerves which compose the great sympathetic cord and those which are distributed to the viscera and arteries. Professor Treviranus of Bremen, Dr Philip, &c. have directed attention to other differences between the nerves of the sympathetic system and those of animal life. The former, remarks the first of these writers, are not at what he calls their origin in the spinal marrow, as intimately united together as the other nerves, and particularly those of the sensorial organs. After their emersion from the bony cavity the reverse takes place. No nerve of a sensorial organ unites during its course with any heterogeneous nerve, except in animals of the lowest grade. *All* the nerves of vegetative life, on the contrary, unite together during their course by means of plexuses and ganglia. This union becomes the more intimate and frequent in proportion to the complication in the organization of the animal. The sensitive and motive nerves also unite frequently during their course; but this union takes place most frequently by anastomoses and plexuses; but rarely by means of ganglia. This law has been long established and developed by Scarpa. Treviranus adds that all the nerves which concur in the functions of the great sympathetic unite with it by means of anastomoses, plexuses and ganglia. If to the differences that are thus shown to exist between the various parts composing the ganglionic system of nerves and those that belong to the nervous system of animal life, we can add some resulting from a difference of origin and functions, we shall find little difficulty in joining in opinion with Bichat,

and the other anatomists who regard the two systems as totally distinct from each other. If we cannot show such differences of origin and function, those which we have seen to be admitted in reference to their anatomical character, can only be considered as establishing the existence of a slight variety in the two systems of nerves, and the whole of Bichat's ingenious theory will fall to the ground.

Before entering on these questions, however, it becomes requisite to say a few words in relation to the evolution and development of the ganglionic system in the human subject, and to point out briefly the characters it presents in animals of an inferior order; or more properly, to ascertain to what portion of the nervous system of our species the nerves found in the lower orders correspond.

Bichat and many other anatomists who have directed their attention to the subject of the anatomy of the nervous system, have noticed that the ganglionic system is slower in its development than the nervous system of animal life; but Béclard, in his additions to this author, remarks (vol. i. p. 354,) that the sympathetic arises at the same time as the brain and spinal marrow. It is found at the third month of fœtal existence. The cœliac ganglia and the splanchnic nerves are developed a little later than the cervical ganglia and the cardiac nerves. Bichat remarks, that the ganglions do not grow proportionably to the organs they supply with nerves, those supplying the genital organs, which are almost forgotten in the early period of general nutrition, being as large as those which furnish branches to the liver, the stomach and intestines—organs that are characterized by their early growth. These nerves, according to the same authority, observe, in respect to growth, the same laws as the ganglions, although the arteries on which they ramify are more or less developed according to the organs into which they penetrate.

This opinion, however, though generally received at the present day, is far from having been adopted by all anatomists. Already Béclard himself, who, as we have seen, admits that the sympathetic arises at the same time with the brain and spinal marrow, after remarking that at the first month of fœtal existence the intercostal spinal nerves of the vertebral animals are observed, (consequently before the brain and nerves), adds, that

the filaments of the sympathetic, owing to their transparency, may possibly escape at this period the notice of anatomists. Other writers have been more positive on the subject. Ackerman concludes, for example, that the sympathetic nerve is necessarily the first part found, because the heart, in which the vital energy prevails in the highest degree, is the centre of organic life. A view of the subject, nearly similar, has been adopted by Mr Parker, some of whose interesting lectures on "Comparative Anatomy as illustrative of General and Human Physiology," delivered in the medical school of Birmingham, have lately appeared in a British contemporary journal. His reasons appear to be chiefly drawn from facts presented by the anatomy of the nervous system in the lower animals. At the earliest period of animation, Sœmmering has considered, says Mr Parker,

"The nerves arising from the ganglionic system to be peculiarly the nerves of the heart and arteries, endowing them with that irritability which presides over all their phenomena, in health and in disease. This observation likewise derives support and illustration from what we perceive in the progress of the development of the chick—a true sanguineous circulation, commencing as early as the twenty-fourth hour of incubation: the heart and large arterial trunks are in action towards the end of the second day, after which period the increase of the arterial and venous systems, lining the chorion, exceed all calculation. That this system of nerves is first formed, first in action, presiding particularly over the functions of the vascular system, must be evident from the mode of life carried on in the fœtus during gestation. 'As soon as the least organization is apparent, the little heart may be seen producing its blood on all sides; the heart is the part first formed, and as all the organic phenomena depend upon it, we may readily conceive in what way the functions of inward life are thrown into exercise.' "

But be this as it may, the system of nerves in question appears at a very early period in front of the spinal column, under the form of small ganglia, communicating with each other, and with the spinal centre, by means of medullary cords. It is from this circumstance that some writers have been led to conclude, that the sympathetic nerve retains much longer, even during the whole course of life, the distinctive character of secondary forms; since the different gangliform masses which constitute it do not agglomerate into one mass, and hence present a series of

distinct organs, more or less approximated to each other, as is observed to obtain in the nervous system of invertebral animals. It is not to be forgotten, at the same time, that this incomplete union may serve as an argument in favour of the opinion that the sympathetic is formed later than the brain, —the parts which constitute it being more widely separated than those which concur in the formation of this organ.

At the period when first distinctly seen, the sympathetic ganglia and nerves are of the same colour as they are subsequently in the adult. They are even at this period proportionably larger than in after life, with the exception, as we have seen, of the semi-lunar. In old age they become paler and dryer than they were at an earlier period, or in the prime of life. The filaments appear less numerous and smaller, so that this atrophy seems to coincide with that sluggishness and diminished energy of the organic functions generally observed in individuals far advanced in life.

While on this subject it will be proper to notice a circumstance in connexion with the development of the sympathetic system, on which a good deal has lately been said by comparative anatomists. We have already stated the views of Ackerman, Sœmmering and others, relative to the connexion of the sympathetic system with the heart and arteries. Mr Serres has extended the application of this anatomical and physiological fact to the development of this system of nerves. It results from his researches, that in the four classes of vertebral, and in the invertebral animals, the evolution of the sympathetic is in proportion to that of the sanguiferous system, and that, on the other hand, the sympathetic decreases in the same ratio as the latter. Thus, as we descend from the mammalia and birds to the reptiles and fishes, we find that the sanguiferous system becomes more and more atrophied. The same, as we are informed, occurs in relation to the sympathetic.

With a view of elucidating the intricate subject of the physiology of the ganglionic system, comparative anatomists have referred to the organization of the nervous system in the various classes of the lower animals, and noted the correspondence existing between the organs developed in each class, and the number, size, &c. of the ganglia, as well as of the filaments with which their organs are furnished. But the discrepancy

of opinion on the subject of the proofs furnished by comparative anatomy is no less evident than on that of the structure of the sympathetic, arising from the difficulty of ascertaining what should or what should not be regarded as that nerve in an animal occupying a lower rank in the scale of animated beings; in other words, to what set of nerves in man those found in the lower orders correspond.

It would occupy more space than we can conveniently allow, on the present occasion, to offer in detail the various opinions that have been advanced on the subject of the nature of the nervous system of the lower orders of animals. Suffice it to state, that while some anatomists regard the ganglia and nerves of the latter as corresponding to the ganglionic system of the higher orders, others maintain that they constitute the rudiments of the spinal marrow; others again compare them to the spinal ganglia, and another set refer them to the rudiments of the eighth pair. Taking a comprehensive view of all that has been said on the subject, and particularly by the most recent and able investigators, we should, were we called upon to form an opinion, conclude that there are some classes of animals in which none of the nerves of organic life can be found; consequently, that the opinion of those who contend for the invariable existence of the ganglionic system in all classes of animated beings, is not fully established. We also draw the conclusion from the researches to which we allude, that the ganglionic system, instead of acquiring predominance in proportion as we descend the scale, and finally forming alone the nervous system of the lower animals, as is maintained by some writers, on the contrary, diminishes in the number and size of its ganglia and cords. Already Béclard, from the researches of others, as well as from his own, had drawn the conclusion that in all animals which, like man, possess a medullary cord enclosed in an osseous canal, the system of ganglia is not developed to the same extent: 1, In having regard to the total size of the body, this system is found to acquire greater extent, in proportion as the animal is more elevated in the scale of animated beings; consequently, in proportion to the degree of perfection in the organization of the latter; 2. Its development is always proportionate to that of the medulla, if we compare both in that respect to the whole

body in general, and not to the brain alone; 3. The great sympathetic follows, also, in its growth, the development of the alimentary canal; 4. Finally, the sympathetic system is likewise connected, in respect to development, with the vascular system.

A circumstance which seems to throw more light on the functions of the sympathetic system consists in this, that fœtuses have come to the world destitute of cerebrum, cerebellum or medulla spinalis; and yet have appeared to live to the last moments of utero gestation. Cases to that effect are recorded by Morgagni, Wepfer, Ruysch, Kerkring, Littre, Sue, Fauvel, Mery, Huber, Lallemand, &c. In most of these instances the fœtus was well formed, so that the processes of nutrition and circulation must have continued undisturbed, and the child must have died, at least in some cases, after birth, from the inability of effecting the respiratory movements which require the agency of the cerebro-spinal nervous centre. In such of these infants as were dissected, and whenever due attention was paid to the ganglionic system, it was found perfectly well formed and developed, and, according to Serres and other anatomists, even more so than in other fœtuses,—a circumstance which indicates that these nerves were adequate to the performance of their functions, whatever these may be. But as we shall have occasion by and by to draw some deductions for the coincidence of the continuance of certain functions with the development of the ganglionic system, and the absence of the cerebro-spinal centres, we shall not dwell at present on these anatomical anomalies, but proceed to offer a few remarks on the origin of the sympathetic system.

The opinions on the subject are, as on many other points connected with the ganglionic system, greatly diversified. By the old writers,—so early indeed as the time of Achilinus and Eustachius, this nerve was denominated *intercostal*, and was regarded as *cephalic*, and as arising from the fifth or sixth pairs, or from both of these conjointly. This opinion was, however, after some time abandoned, and gave way to the idea that the sympathetic system of nerves takes its origin in the spinal marrow; and that instead of deriving its original branches from the fifth and sixth pairs, it sends filaments to these nerves. So far as we can recollect, Petit was the first to advance

this opinion relative to the nature of the correspondence between the ganglionic and cephalic nerves. He was followed by many of his contemporaries and the greater number of his successors, and fully substantiated by Fontana and Girardi. The spinal origin of the sympathetic, and the rejection of the idea that it arises from the cephalic pairs we have named, was early supported by the following reasons: 1. It was remarked that the communication of the former with the branches of the fifth and sixth pairs, takes place at an acute angle pointing posteriorly—a circumstance proving, it is said, that the nerve in question, instead of arising from the fifth and sixth pairs, ascends towards them. 2. That the sixth encephalic pair is larger after receiving the filaments of the sympathetic than before that junction; which tends to establish the fact that the former does not *furnish* but *receives* filaments from the sympathetic. 3. Finally, that the great sympathetic nerve diminishes in size as it approaches the cranium, which is exactly the reverse of what ought to take place did this nerve arise, as was maintained, from the fifth and sixth pairs.

Some time after Petit, Lancisi, Le Cat, Johnston, Winslow, Girardi, and others, described the sympathetic as an isolated system of nerves,—many of them representing the ganglia, which in great part compose it, in the light of small brains, and the filaments uniting these ganglia above to the fifth and sixth encephalic pairs, and all along the spine, with the nerves arising from the medulla, as simple anastomosing branches. This opinion was subsequently adopted by Unzer, Metzger, Hufeland, Proshasca, Sue, Bichat, Reil, Gall, and through means of their influence, by the greater number of succeeding anatomists and physiologists. According to this view of the subject, then, the sympathetic system is composed of a series of ganglia, constituting so many nervous centres, each independent of the other, but all communicating, by means of intermediary filaments, with one another, with the spinal marrow and with the brain.

The arguments and anatomical facts adduced, particularly by Bichat and Reil, in support of this opinion, may be summed up as follows. 1. The branches passing from the spinal marrow to the ganglia of the great sympathetic, redden, and assume gradually, and in proportion as they approach the gan-

glia, all the characteristic features of that nerve; from which we may conclude, remark these distinguished writers, that those nerves are rather branches anastomosing with the medulla, than filaments arising from the latter and giving origin to the sympathetic. 2. The branches passing from one ganglion to another, are in turn single and multiple, small or large; a disposition which does not accord well with the idea of all these branches being a single and continuous nerve. 3. There exists no proportion between the filaments sent off by a ganglion at its lower portion, and those that enter into it from above,—a circumstance which does not favour the idea of the former being the mere continuations of the latter. 4. It is evident that each ganglion, as well as the nerves it furnishes, has their proper texture, by which they are distinguishable from all the others. 5. There often exists interruptions in the course of the sympathetic. Haller, for example, has known it to terminate at the sixth rib, and reappear at the seventh dorsal nerve. Bichat remarks, that it is often interrupted between the last thoracic ganglion and the first lumbar, and between the last lumbar and the first sacral.

It may also be added, that if the sympathetic system arose, or received all its force from the spinal marrow, its anastomosing filaments would not, in their progress to that centre, supposing them really to join it, have been placed in communication with branches and ganglia that belong to the sensitive set of nerves, and which consequently carry their nervous power *towards* the spinal marrow; and secondly, that the mode of junction of those filaments with the spinal nerves, so far from being calculated to make us believe that the former proceed from the medulla, has induced some to think that they do not even join that centre, the intercostal nerve passing the ganglion a considerable space before being joined by the filaments of the sympathetic, which assume with it an outward direction.

Besides these, other reasons, founded on physiological phenomena;—the independence of action of the ganglionic system, &c. have been adduced by the supporters of this opinion. But we postpone the mention of them until we come to speak of the uses and functions of the ganglia and nerves of that system. It is not to be presumed, however, that the opinion we have been considering has been adopted unanimously, and without

opposition. Chaussier, for example, continued to the last to consider the sympathetic as an encephalic nerve; J. F. Meckel regards it as at the same time cephalic and spinal;—placing its origin in several cerebral and in all the spinal nerves; but, nevertheless, considering it as a centre of action independent of the brain and spinal marrow. For this disposition, or property, it is indebted to its numerous points of origin, and to the number of the plexuses it forms; for it is proper to remark, that, agreeably to this distinguished anatomist, the independence of any nerve of the influence of the cerebro-spinal centres, as well as of that of the power of volition, is found to be in direct ratio to the number of points of origin, the number of its plexuses, and of the ganglions through which it passes. A view nearly similar to that of Meckel's is held by professor Treviranus.

Le Gallois, Philip, Béclard, Olivier, Bégin, Gama, Lobstein and others, on the contrary, believe it to be a spinal nerve, and refuse to consider the ganglia as separate little brains. Le Gallois and Philip's reasons are derived, not so much from anatomical peculiarities, as from physiological phenomena, resulting from their experiments, and which led them to adopt certain opinions relative to the functions of the spinal marrow, and its influence on organs supplied with nerves of the ganglionic system, as we shall have occasion to notice hereafter, and to affirm that it is the particular character of this nerve to put all the parts to which it is distributed under the immediate influence of the whole nervous system.—Opinions, however, which did not prevent them from recognising that those parts are by the same cause removed from under the control of volition.

Béclard remarks, that some rare and perhaps badly observed interruptions in the trunk of the sympathetic nerve, have led some anatomists to regard the existence of this trunk as a circumstance of little importance. He considers this opinion as erroneous, but adds, that the *roots* of this nerve (the sympathetic) are evidently to be traced to the spinal nerves, and not to the fifth or sixth pair of encephalic nerves. (General Anatomy, §. 808.) In his additions to Bichat, he observes also, that all that has been said to prove that the ganglia are nervous centres, only shows that there is a great difference of organization between the sympathetic and the other nerves. Mr Craigie in

his General and Morbid Anatomy shows, that at least one part of the opinion of those who contend for the independent origin of the sympathetic system, viz. that the ganglia are small brains,—the originality of which opinion, we must be allowed to say, en passant, he wrongly attributes to Johnston, forgetting that Lancisi, Petit and particularly Le Cat, who certainly preceded Johnston, entertained very similar views on the subject; he has shown, we repeat, that such an opinion respecting the nature of the ganglia is, strictly speaking, incorrect, the anatomical structure, and he might have added, the chemical composition of these parts, being quite dissimilar.

Professor Lobstein affirms, in support of the spinal, and against the ganglionic origin of the sympathetic, that he succeeded in tracing the nerve *through* the cervical, as far as the thoracic ganglia; the nerve in this passage mixing, it is true, with the proper substance of the latter; but, nevertheless, remaining sufficiently distinct to be readily recognised. The same anatomist, moreover, remarks, that he has traced the roots of the great sympathetic as far as the first cervical ganglion.

In support of the opinion of the spinal origin of the sympathetic, it has been contended that some facts, first pointed out, we believe, by Meckel and by Weber, in a memoir on the subject of this nerve, published at Leipsic in 1817, and repeated by Béclard and Lobstein, may be adduced, that the development of the nerve in question is always in direct ratio to that of the spinal nervous centre; that in fish, in which the true medulla first appears, the sympathetic is found in a very imperfect state, while on the contrary in all animals in which the medulla is large, it presents a corresponding increase in bulk; finally, that on examining the sympathetic in the various classes of animals, we discover that it diminishes in size and extent in an inverse ratio to the pneumogastric or par vagum, (8th pair), in such a way, indeed, that in some animals the latter assumes altogether the place of the sympathetic. Can we not admit, it is said, from this last circumstance, that the functions of the two nerves being in some classes similar, their origin should be also considered as the same; in other words, that the par vagum originating in one of the great nervous centres, the sympathetic should be traced to the same centre,

or its spinal prolongation. But Treviranus, who considers that all nervous ramifications arise from true ganglia,—and by these he means those in which there is a complete decomposition of these fibres, is disposed to think that the functions which Weber attributed to the par vagum were, in reality, performed by the sympathetic, rendered, from some anatomical peculiarities, more subject to the influence of the sensitive sphere (action of the nervous centres of animal life) than it is in mammiferous animals. "Copulation in fish without sexual intercourse, is a fact which it is difficult to explain, unless we admit that there take place in them certain impressions which, operating solely on the olfactory and optic nerves, that is to say, on the superior sensitive sphere, are capable of producing in them, without the immediate excitation of the genital organs, the excretion of the eggs in the female, and of the semen in the male; and, consequently, that their great sympathetic nerve, at least in what relates to the genital parts, must be less independent of that sphere than it is in the superior classes of animals. In reference more particularly to Weber's opinion, Treviranus remarks, "with the exception of the great cord of the nervous vagus, which in fish passes down on each side of the body, the ramification of this pair takes place in the same manner as in the superior animals; but this distribution would necessarily be very different, if the whole of the vagus nerve acted as the *vicar* of the great sympathetic. The lateral cord already mentioned is the only one which might be supposed to exercise a function entrusted to the sympathetic in the superior classes. But its course and its ramifications induce us to suppose, that it only presides over a function peculiar to fish, rather than that it exercises an influence over a function common to fish and to the superior animals, and over which, in the latter, it is only the sympathetic that exercises any influence." According to this author, the large size of the par vagum in fish, compared with that of the sympathetic, depends on the greater energy required in organs which the former supplies than in those entrusted to the influence solely of the latter*.

* Mr Serres thinks that the branch of the eighth pair, which Weber regards as the vicar of the sympathetic, is analogous to the recurrent nerve of Willis, the

The question relative to the origin of the sympathetic nerve is not one of mere curiosity, for it involves that of the origin of the nervous power of that system. But whatever view we may adopt relative to the derivation of the nervous influence of the sympathetic;—whether we admit that it comes wholly or only in part from the spinal marrow, we are not sure that we can be justified in saying that the sympathetic *arises* from the spinal marrow; and it is but justice to remark, that several of the anatomists and physiologists we have found to advocate this origin, have not understood a physical one, that is, that the sympathetic *grew* from the spinal marrow, but merely wished to convey the idea that the nerve in question derived, to a greater or less extent, its power from that centre. We believe that all modern investigators into the anatomy and physiology of the nervous system of animal life agree in the opinion, that it is improper to refer the origin of the cerebro-spinal nerves to the great nervous centres; in other words, they deny that those nerves proceed *from* these central masses, and contend, that they are all formed at the same time with the latter, proceed towards them, and unite with them some time after the first development of the embryo. We might, a priori, conclude, therefore, that the same law of organology obtains in respect to the sympathetic, did we not possess positive proofs, derived from the anatomical researches of very high authorities in our science, of this simultaneous origin of the different orders of nervous cords and ganglia;—of the sympathetic observing the same laws as other portions of the nervous system;—of its originating in the parts in which it is found;—following the development of these parts;—disappearing in monsters by default of parts, and multiplying in those that present a multiplicity of parts;—of its growing *toward* the great centres, and of its placing itself in relation with them like every other nerve.

The principal point then in this inquiry is to ascertain whether really the sympathetic system possesses the power of acting independently of the nervous system of animal life—of the

development of which has attained its maximum, owing to the greater respiratory power required in this class. He rejects the views of Weber in respect to the vicarious action in question, owing to the relationship existing between the sympathetic and the vascular system, to which we have already alluded.

brain and spinal marrow—or whether it derives its energy from the latter. This will best be done by inquiring into the functions of the ganglia and nerves separately; though by so doing we are not prepared to draw a line of demarcation between the functions of these parts; in other words, of separating them into two systems, instead of regarding them as two portions of the same system.

Are the ganlia and nerves of the sympathetic system endowed with sensibility? This question deserves to arrest our attention a few moments, because on the manner in which it is answered will depend the solution of a point of some interest in a physiological sense. M. Magendie states that, having cut, pricked, and torn the sympathetic ganglia of the neck in dogs and horses, he found that the animal was not conscious of the operation; in a word, felt no pain. From this he concludes that these parts are not endowed with sensibility, by which he means, no doubt, animal sensibility. Long before his time Bichat, Wutzer, and since them Lobstein, had gone far to show that the symphathetic is insensible to mechanical irritations. Haller had, it is true, found, on one occasion, that pain was excited in a dog by irritating the hepatic plexus: but still many were inclined, from a recollection of the results obtained in the experiments of the physiologists we have cited, to attribute the effect in this case to some accidental cause, rather than to the true sensibility of the nerves. Within a few years the experiments of Mr Flourens seem to have thrown some light on that subject. From these experiments, which it is useless to detail in this place, we may conclude with M. Flourens, 1. That the semi-lunar ganglion is always, and very, excitable; pain having always been the result of its mechanical irritation. 2. That the other ganglia are also excitable, but only occasionally so, and even then in a slight degree. 3. That all which the most accurate observers have said respecting the *high nervous power* residing according to them in the diaphragmatic region, and celebrated under the name of *archeus*, *præses systematis nervosi*, phrenic centre, epigastric centre, &c. appears in some measure justified by the extreme susceptibility of the semi lunar ganglia. It is right to remark here, however, that in all these experiments the ganglia alone were irritated, and not the nerves; consequently, that no deductions can be drawn from

them relatively to the sensibility or want of sensibility of the latter.

Mr Flourens it is true concludes, moreover, that the effects thus produced,—this faculty of transmitting pain which appertains to no part except the nerves of the spinal marrow and brain, demonstrates in a positive manner the intimate union existing between the latter and the sympathetic; but we are yet to be shown that the sensation of pain was conveyed exclusively by the nerves of the sympathetic, as we see no reason to refuse admitting that the ganglia, or at least their coverings, are supplied with nerves of animal life. We have reason to believe, indeed, that the nerves of the sympathetic in their natural state are not endowed with that kind of sensibility which is perceived by the sensorium, and is called animal. But, at the same time, we believe, that under particular circumstances they become the seat of pain. We perceive every day that inflammation of parts which in the healthy state are insensible, produces great pain. Gastralgia may be sufficient for an example. But even in the natural state they are the means of conveying sensations to the brain, which call forth in that organ movements of various kinds; or simply to those parts in the neighbourhood of those on which the impression has been made.

It is perfectly well known, from the experiments of Fowler, Rinhold, Webster, Nysten, and of Mr De Humboldt, although Mr Lobstein has failed in their repetition, that the galvanic fluid produces an effect on the sympathetic system, and through this on the parts it supplies with nerves. No pain, it is true, seems to have been produced by these experiments, so that it will be unnecessary to refer to them particularly. Yet they tend to show that the nerves of the sympathetic convey sensations or impressions, and are consequently irritable. Mechanical irritations show the same, and Magendie relates, that having divided the two cords of the par vagum of a dog, he introduced some tartar emetic into the stomach of the animal; in five minutes after vomiting took place,—which proves, according to Magendie, and we would suppose, according to every one else, that the sympathetic had conveyed the sensation which caused the sensorium to command the movements necessary for the accomplishment of that effect. Indeed we might, a priori, and without waiting for such experiments, conclude that the sympathetic nerves, though

conveying, in the natural state, no sensation of the acute kind, do convey *some* sensations, since the parts they supply with nervous branches receive, so far as we know, but few, some very few, if any, twigs from the cerebro-spinal nerves, and yet are in direct and close relation with the cerebro-spinal centres, transmitting to them commands necessary for the execution of required movements. We may find illustrations of this fact of nerves having the power of conveying sensations, and yet being insensible, using that word in its common acceptation, in the nerves of the third, fourth and sixth pairs, the portio dura of the seventh, though in a less degree than the preceding; the hypoglossus nerve; the anterior branches of all the nerves arising from the spinal marrow; the optic, olfactory, and acoustic nerves, all of which possess sensibility; since they are enabled, some to convey to the centres peculiar sensations, others to various parts of the body, the commands of those centres; and yet some of them when pricked or cut convey no pain, others very little. All we can conclude from these facts is, that the sympathetic is endowed, like the other nerves we have enumerated, with a special kind of sensibility, by which it is enabled to convey sensations to the centre without awakening the consciousness of the individual; in a word, that it is endowed (and consequently endows parts to which it is distributed) with that kind of sensibility which Bichat has called organic; but that when in a diseased state it acquires to a certain extent the real animal sensibility, and conveys painful sensations to the mind.

From his experiments on the ganglia, (by the removal and irritation of which, as we have seen, no pain was produced), and from other circumstances, such as the want of analogy in point of colour, form, consistence, disposition, structure, textural or chemical properties between the sympathetic nerve, considered *en masse*, and the nerves of animal life, and at the risk of contradicting his own admissions, that the sympathetic conveyed to the sensorium the sensation of the impression produced in the stomach by tartar emetic, and that some other nerves are deprived of animal sensibility, M. Magendie concludes that the great sympathetic is not a nerve. (Note to p. 171, vol. i.) If it be not a nerve, what is it? This question M. M. does not pretend to solve, and remarks, that it would

be wiser, and particularly more useful to the progress of physiology, to confess, that in the present state of our knowledge the uses and nature of the sympathetic are entirely concealed from us. It is well to add, before we proceed any further, that M. Magendie seems disposed to doubt that the doctrine which attributes to the sympathetic an agency in the transmission of internal sensations, has any foundation in truth; and, secondly, that his view of the non-nervous nature of that system is also based on this, that in his experiments the parts supplied with filaments from extirpated or wounded ganglia were not affected.

In opposition to this view of the subject, and to the facts on which it is founded, we have only a few words to say.

1. We are not justified in denying the nervous nature of the sympathetic on the ground of the want of resemblance between it and the *real* nerves; for independently of the fact, that some respectable anatomists contend that this difference is not as great as M. M. appears to think, and that there is considerable analogy at least between the splanchnic cords and the cerebro-spinal nerves, and between the small ramifications of the sympathetic and those of the pneumogastric, it is not greater, we mean in appearance, than that existing between the acoustic, optic and olfactory nerves,—and between the portio molis and portio dura of the seventh pair. And yet no one thinks of denying the nervous nature of either of these cords that differ most from the rest.

2. If the ganglionic or sympathetic is not nervous, how comes it that nature has caused it to communicate with the cerebro-spinal centres by means of filaments. We see no tendinous, muscular, bony filaments—no substance of any kind with which we are acquainted, in the system, anastomosing with, or running into the nerves, except nerve. This alone would lead us to *doubt* whether M. Magendie has any reason to *doubt* on the subject.

3. Nor can his scepticism be justified in regard to the insensibility of the ganglia and their nerves; his experiments on the cervical ganglia of dogs and horses merely show that these ganglia are insensible; and surely from this fact no one is authorised to conclude that *all* the ganglia are insensible. We have already stated that M. Flourens has

found the semi-lunar ganglion to be always very sensible when pricked or irritated by any mechanical means. Our second reason is, that were we to consider the want of sensibility in a ganglion and nerve as a proof of such parts not being *nervous*, we should be forced to admit that cords, and other parts, which every one, even M. Magendie himself, considers as nervous, are not of that nature; since, according to his own showing, they do not convey painful sensations when pricked, cut, &c. Our readers will recollect what we have said, but a few pages back, of the insensibility of the optic, acoustic and other nerves,—a fact which, indeed, we stated principally on the authority of M. Magendie. To this may now be added, that the cerebral and cerebellic hemispheres have this analogy with the former, that they do not convey pain when pricked, torn, cut, and even when submitted to the action of the actual cautery. Will M. M. feel disposed to deny that these parts are nervous? The inference would be as fair in one case as in the other.

4. If the sympathetic is not nervous, it would follow that parts in which no nerves of the cerebro-spinal system have been traced, and which, in all likelihood, possess none such, have no nerves at all. In this case, how are the sensations arising or produced in them conveyed to the centres? We know of no mode of communication by which this can be effected, except by nerves; and the very fact, that these sensations are conveyed, and that the cords of the sympathetic are the only ones with which such parts are supplied, would be sufficient to show that they are nerves. M. Magendie in this has contradicted himself, for, as we have seen, he admits in another volume of the same work, that emetic tartar, when introduced into a stomach whose nerves of animal life were previously cut, excited vomiting, and he attributes the effect not to absorption, (for he himself remarks, that the time which elapsed between the introduction of the article and the efforts for its ejection, was too short to account for the effect by absorption), but to a sensation conveyed through the sympathetic nerves with which the lining membrane of the stomach is plentifully supplied.

5. If M. Magendie allows thus much in the above case, we can see no reason why he should deny that internal sensations

are conveyed by the sympathetic, although no positive experiments may have shown it to be the case. We have a right to infer it, for our internal sensations are the effects of our wants, and our wants, whether to receive or reject, are themselves the effects of excitations of the membranes of relation. Now what signifies whether the want be produced by tartar emetic or by fœcal matter, urine or any other foreign substance. In all such cases an excitation is conveyed to the brain to elicit there the command of certain movements necessary for the execution of the operation of ejection. If the sympathetic nerve does it in one case, what right has M. Magendie to deny that it does it in the others. The want of air in respiration, of food for the sustenance of the body, of water to quench thirst, always act by creating stimulation of the membranes of relation. Hence arises the internal sensation which is conveyed to the brain. Now if the sympathetic has been found adequate to this conveyance in the case of the tartar emetic, why refuse to admit that it may be equally so in the latter instances? It is no argument to say, that in some of the instances in which the sensations produced by the wants, nerves of animal life may serve to account for their conveyance; for in the instance recorded by Magendie, the nerves of the eighth pair had been cut, and there remained to account for the phenomenon no other nerves than the sympathetic. The question is not what part the eighth pair would have played in the effect in question, had they *not* been cut, but what was the means of transmission; and whether that nerve would have been the sole agent or not, in the natural state, matters not at present: all that is necessary to settle being simply, that in some cases the sympathetic has effected the transmission, as this alone is sufficient to prove that it is a nerve, and that M. Magendie has been found guilty of contradiction.

6. Let us admit, for the sake of argument, that the sympathetic, in the natural state, has no sensibility of any kind, consequently that it does not convey sensations; in a word, that it is not a nerve. How shall we account for its sensibility in a diseased state? A part which is sensible in a healthy or diseased state and is not nervous, must be supplied with nerves; sensibility being the appanage of the nervous system. Now where are the nerves *supplying* the sympathetic and endowing it with sensibility when in a diseased state? This

affords us the opportunity of pointing out another inconsistency in the work of M. M. He remarks, at page 170, vol. 2, that "the intestines acquire sometimes, especially when in a state of disease, a very high degree of sensibility, and become often the seat of excruciating pain. As they hardly receive nerves from the cerebral pairs, it is probable they owe their sensibility to the filaments of the great sympathetic, although no direct experiment has yet proved it." It is difficult to reconcile this faculty in the sympathetic with the idea of its not being a nerve.

7. The argument founded on the circumstance, that in the experiments made by M. Magendie, the functions of the parts supplied by the branches of the ganglionic system were not impaired, is not entitled to much attention, though in some measure supported by the results obtained by Edwards and Vavasseur; for they are contradicted by those of Brachet, Dupuy, Enns, &c. as well as by phenomena observed in diseases of the ganglionic sytem.

Scepticism, when restrained within proper bounds, is, in matters of science, as indeed in every other, highly commendable. In physiology and pathology particularly, we are the first to recognise the absolute necessity of a due proportion of it; for he who gulps every thing he hears or reads, is not worthy of being styled a philosopher, the province of whom is to arrive at truth by a severe scrutiny of facts; and he runs the risk of adopting, as truths, many things that are the result of sports of imagination, and which may lead to an erroneous treatment of disease. But in these sciences, as in every other thing, we may carry this spirit of scepticism too far, and will, whenever this is the case, often deny the accuracy of things which afterwards are recognized to be substantial facts—thus remaining in arrears of others, allowing ourselves, in fact, to be dragged along by the current of science, not keeping pace with it, and still less giving it an impulse. If we inquire who those were who contributed,—who those *are* who still contribute,—to the advancement of science, we shall find that in no instance did they belong, or do they belong, to this class of sceptics; and certainly, if M. Magendie deserves to be considered in the light of a promoter of science, it is not on those points in which he has been so backward in believing, as he

appears to be on the occasion of the nervous nature of the sympathetic. There are many points in physiology that are recognized by the soundest minds as demonstrated truths, though no direct experiments may have been made to lead to this conclusion, while a great many are recognized as truths founded on experiments, which are still doubted by minds of equal solidity. This shows that mere experiments on living animals are often fallacious; and even supposing they were not so, at least they are not the only means we possess for arriving at a knowledge of the nature and functions of the various general tissues, as well as of the organs of the body. If M. M., therefore, believes only what his experiments show him, or what he *thinks* they show him, to be facts, and *doubts* every thing else, he will, we fear, often establish conclusions which will be as often incorrect as otherwise, and disbelieve things which every one else as clear-sighted as himself recognizes as truths, thereby allowing himself to be led often *ad absurdum*, and deserving of being classed with certain men who constitute the rear-guard of the legion of savans ;—in a word, he will be in physiology what Pinel was in pathology, one of the real Wouter Van Twillers of the science.

Assuming then as a demonstrated fact that the sympathetic or ganglionic system is *nervous*, we proceed to inquire into its functions.

Of the ganglia.—Willis, Petit, Le Cat, Winslow, &c. considered, as we have already seen, these bodies in the light of small brains, whose office is to secrete the animal spirits. Lancisi and Vicq D'Azyr regarded them as the centres of impulses, and compared them to the heart, supposing that they act by promoting the transmission of those spirits, or as reservoirs in which these are kept in readiness for the supply of the demands of the body. Johnston entertained an opinion nearly similar to that of the first set of physiologists, and believed that the office of the ganglia consists in impeding the transmission of sensations, as well as of the influence of the will in the nerves that emanate from them;—an office essential for the regular and independent play of the functions of certain organs, as the heart for example. But he held also that these little brains are capable of developing and communicating nervous influence, and give origin to the involuntary nerves. Others affirm, on the contrary, that they serve to concen-

trate and reinforce nervous action, or distribute it uniformly to the several apparatus of the nutritive functions. Scarpa, adopting in some measure the opinion of Meckel and Zinn, thought himself justified in the belief, that the ganglia are destined to divide, mix, and reunite the nervous filaments. "According to him, all the visceral nerves arise directly from the spinal nerves, and from the fifth and sixth cerebral pairs, and are merely united in the ganglia" to be thence distributed. Bichat, Reil, Gall and many of their successors and contemporaries, modifying and extending the views of Johnston, regarded the ganglia as the centres of as many particular nervous systems, and as analogous to the ganglia which, according to them, compose alone the nervous system in animals in which the latter presents the greatest simplicity. Dr Lobstein also, in his elegant work on the sympathetic, joins in this belief, regarding the ganglia as the laboratories of the nervous principles distributed to the organs over which they preside.

From this it appears, therefore, that, making allowance for certain peculiarities of views existing at various times, and arising from the manner in which the operations of the economy were explained, the opinions relative to the functions of the ganglia may be arranged, as Béclard has remarked, into two classes. In the first will be placed that maintained by Meckel, Zinn, Scarpa, &c. by whom these functions are thought to be limited to a mechanical process—that of facilitating the distribution, &c. of the nervous fibres. In the second class we place those of Willis, Petit, Le Cat, Johnston, Bichat, to whom may be added Reil, Wutzer, Lobstein, Broussais, &c. by whom functions of a more elevated order,—essentially vital, as we have just seen in detailing those opinions separately,—are attributed to the ganglia.

The arguments adduced in support of both these opinions seem to us to be worthy of attention; for on a careful investigation of the subject it will be found, that neither of them can be regarded as sufficient, if adopted, to the exclusion of the other. In a word, there are phenomena which go to show that the ganglia act, or seem to act, in a manner independent of the cerebro-spinal centres; while it is evident that certain impressions starting from these centres penetrate through the ganglia, and influence, to a greater or less extent,

the action of the organs that are supplied with filaments from the sympathetic.

That the opinion of Johnston and Bichat is not destitute of foundation, we may infer from this circumstance, that the same relationship which exists between the cerebro-spinal centres, and the nerves that branch off from them, be they of motion or of sensation, is observed to exist between the ganglia and the sympathetic nerves; namely, that as the nerves of the former system diminish in size as they recede from the brain and spinal marrow, so the nerves of the sympathetic diminish in size as they recede from the ganglia. We find, also, that the action of the cerebro-spinal system is not necessary for the exercise of certain internal functions, since these functions are performed in the fœtal state, when the nerves of animal life and their centres are quiescent, and even in cases in which the latter are totally wanting,—the child coming into the world, nevertheless, fat and well made, and his circulations having continued to the last moment; circumstances which, as we have seen, show that the nutritive process, as well as the action of the heart, arteries, and capillaries, have remained unimpaired. During life, also, we find that the operation of organs supplied with ganglionic nerves continue unobserved by the cerebro-spinal centres, and without in the least affecting these; that impressions made upon these parts create, in the natural state, no pain; that common stimuli applied to the nerves of animal life do not influence the others; that after destroying the spinal marrow in living animals the action of parts supplied with ganglionic nerves may, by some means we shall mention by and bye, be made to continue, &c.; that the structure of the ganglia of the sympathetic are somewhat different from that of the other ganglia, from which must result a difference in the functions of each class; that during sleep and in apoplexy, the functions of parts receiving nerves from the ganglia continue though the action of the other system of nerves be suspended; that in all cases, parts supplied with nervous power by the sympathetic are placed beyond the control of the will.

The nerves, besides, which supply these parts, arise from the ganglia; hence it is fair to conclude, that in those cases in which the spinal marrow and brain did not exist, or was de-

stroyed, the nervous energy was derived from the ganglia; since it must be derived somewhere, and cannot be supposed to be furnished by an organ which no longer exists, or has never existed ?

Perhaps, also, the results of experiments on the ganglia, and the phenomena observed in diseases of these parts, could be cited in proofs of their great influence in sending through their nerves a degree and kind of innervation peculiar to themselves. The reader need but refer to the younger Le Gallois's paper on the subject of the diseases of these ganglia, contained in the Revue Médicale, and in which, independently of some interesting cases, he will find an account of M. Dupuy's experiments; to Mr Teale's work on neuralgia, and to M. Brachet's work on the functions of the ganglionic system, to be convinced of the power these ganglia have of developing nervous influence.

In support of the other opinion, it is alleged that the independence of the ganglia is not so absolute as is maintained by the supporters of the theory of Johnston and Bichat, since the effects of impressions originating in the cerebro-spinal system, as passions of the mind, principally, are transmitted to organs regarded as exclusively under the control of the sympathetic—the heart for example; that impressions made on the gastric surface, whose nerves are furnished by the ganglions, are conveyed to the brain and spinal marrow, as proved by the fact that certain movements, which it is not necessary to specify, are called into play, even after the division of the nerves of animal life; that these surfaces become in some instances the seat of pain, which indicates that the brain has received an impression. By some it has further been objected against the opinion of Bichat, that no reason can be found why the vertebral ganglia should not perform the same office in regard to the parts they supply with nerves; and yet that these spinal ganglia being attached, without a single exception, to nerves which bestow sensibility, do not cut off sensation, as those of the sympathetic system are supposed to do. And Haasen, who regarded the ganglia in the light of plexuses, has objected to the theory of Johnston, on the plea that the voluntary muscles receive ganglionic nerves, and that those of the involuntary muscles often do not arise from those ganglia.

Without pretending to lean more on one side than on the other of the question before us, we must be allowed to remark that the two last arguments we have mentioned in opposition to the independence of the ganglia, are not as conclusive as some physiologists have thought; since the spinal ganglia are not precisely organized like the others, and belong to a distinct system, having particular and distinct functions to perform, and may therefore not cut off sensibility, while the others may be supposed to do so, without our violating the laws of common sense. As regards the objections of Haasen, it may be replied, that the voluntary muscles which receive nerves of the sympathetic kind, receive also some from the other system, and that consequently the latter must be regarded as sufficient to explain the influence of volition over them,—the former serving for different purposes, which will be mentioned in due time; and that the stomach and other parts supplied with the eighth pair, (which, by the way, is so far, by its anatomical and physiological peculiarities, removed from under the influence of the will, as to be regarded by Treviranus and Lobstein as a nerve of vegetative life), are also abundantly supplied with ganglionic nerves.

From all we have seen and read on the subject of the functions of the ganglia, then, we believe they may be regarded as, to a certain extent, centres in which nervous power is developed, and from which it is communicated with that derived from the spinal marrow to the parts they supply with nerves; that they shield the parts they furnish with nervous energy, from the empire of the *will;* that these centres are, in a great measure, subordinate to the great cerebro-spinal centres; that they act in impeding the transmissions of ordinary impressions from the great centres to the organs they supply; but that they are not so isolated as to prevent the transmission of powerful impressions. A few facts will serve to exemplify this. While the ordinary operations of the mind do not affect the involuntary organs, the different passions are however well known to do it, and to a remarkable degree too; the action of the galvanic pile on the lumbar ganglions, in the experiments of Wutzer and others, gave rise to convulsive movements, while a less degree of irritation did not give rise to a similar effect. The ganglia seem even in these cases to act by *diminishing*

the influence of the great centres, and on the other hand, to diminish the force of transmission of an impression *to* the same centres. It is to be borne in mind, however, that, as we have already remarked, the whole mass of ganglia and nerves which are collectively denominated *sympathetic*, do not constitute *one* system, with but *one* function to perform; but that one part belongs, probably, to the nutritive functions, while another is destined for sympathetic or associating purposes; and that consequently the ganglia of each portion have a different function to perform. But this does not affect the question before us; since we may suppose that both are to a certain extent under the dependence of the cerebro-spinal system, though at the same time constituting independent centres. We should suspect, however, that the ganglia of the nutritive functions are more independent than the others, and that the parts they supply with their peculiar innervation are only affected by impressions starting from the cerebro-spinal system, through the agency of the sympathetic portion of the ganglionic system.

This subject will necessarily be alluded to once more, when speaking of the sympathetic *nerves;* to which subject we next turn our attention.

It is evident, that the cords of the sympathetic have for their function the transmission of nervous influence, since they are nerves, and nerves so far as we know have no other function than this transmission. Yet, from what we have seen of the functions of the ganglia, it will readily be inferred that this transmission is not effected exactly as that which takes place through the agency of the nerves of animal life,—with equal certainty under all circumstances, and whether the impression be made with one sort of agents of stimulation or another. In many respects, indeed, the sympathetic nerves may be regarded as very imperfect conductors, compared at least with the cerebro-spinal nerves. Thus it is a fact which experiments have demonstrated, and on which we need not, therefore, dwell long at present, that chemical and mechanical irritations produce no effect upon them, or rather that the impressions such irritations occasion are not propagated through these cords to the parts they supply, while, on the contrary, galvanic irritations are transmitted rapidly through them, and either produce contractions or give rise to sensations of which the individual may be, under cer-

tain circumstances and to a greater or less degree, conscious, and which are very similar to those experienced in morbid intestinal, nephritic, hepatic and similar irritations.

The question, however, is not whether the branches of the ganglionic system transmit or not nervous influence; but what part they perform in the economy by the transmission of this influence. In examining this question we must speak of the ganglionic system as a *whole*, without any reference to each portion of it individually; for it is impossible, in any investigation relative to the function of this system, to separate, even in thought, the nerves from the ganglia; since the cords do not act alone without the agency of the ganglia,—supposing these to be centres of action, or merely reservoirs of nervous influence; and we can readily comprehend, that without cords, the process of innervation could not be accomplished, come the power whence it may. However, in treating separately of the different modifications of functions over which the sympathetic system seems to preside, we shall endeavour to point out the agency of the various divisions of this intricate portion of the nervous system.

The notions of the ancient writers relative to the agency of the glanglionic system in the operations of the economy were necessarily imperfect and unsatisfactory. With slight differences of sentiment among them, they appear to have considered the great sympathetic as being principally useful in establishing the connexions that exist between the various parts of the body; hence the name by which they designated this system. It was not long, however, before physiologists found that such could not be the only function this nerve was called upon to perform; nay, it was even denied that it formed the communicating link between the different organs; because parts sympathise together, although not anatomically connected (at least evidently so), by means of this nerve; and because in many cases a sympathetic union may be destroyed by dividing the nerves of animal life, with which the parts are supplied—the filaments of the sympathetic remaining entire.

Nothing of importance, indeed, appears to have been discovered relative to this important point of physiology until the time of Johnstone, Bichat and Reil, whose views on the subject may, to a certain extent, be inferred from what we

have said of their opinion relative to the function of the ganglia. Observing, 1. That the organs of the involuntary functions, as for example of digestion, respiration, circulation, &c., require, in order to be able to perform with due regularity the important offices with which they are intrusted, the integrity of the nerves distributed to them. 2d. That the nerves of these organs are in a great measure derived from the ganglionic system. 3. That the nerves of the great sympathetic manifest properties very different from those possessed by other nerves; for example, that while these are endowed with great sensibility, the former appear insensible, and may, consequently, be cut and burnt without imparting a sensation of pain;—from a consideration of these circumstances, the two last distinguished physiologists we have named have hazarded the conjecture, that the great sympathetic is charged with regulating those functions in the economy that are independent of volition, and performed without the consciousness of the individual. Hence the name of nervous system of organic life, which the mass of visceral ganglia and nerves attached to them have received, to distinguish it from the other masses and cords which are designated by the name of nervous system of animal life, and are composed of the cerebro-spinal centres, and the cords that emerge from or rather are in direct relation with them; the former being supposed to preside over the involuntary and unfelt functions—over the organic operations of the system; the other over the functions of animal life, or those which, in consequence of establishing a relation with external objects, have been denominated functions of relation.

The opinion of Bichat and Reil has in great measure received the sanction of the physiologists of the present day, who think they have recognized the fact, that the sympathetic presides over the nutritive, secretory and circulatory functions; that it is the regulator or the source of the nervous power which animates the heart, the alimentary canal, the genito-urinary apparatus, and constitutes the bond of union existing between the organs of vegetative and those of organic life;—all of which phenomena are executed with regularity, although the individual has not the consciousness of the fact, owing to the isolation of this system from the general nervous system whence voli-

tion starts, and where impressions converge. M. Lobstein himself, who believes in the identity of the two nervous systems, has, nevertheless, adopted the opinion of Bichat respecting the independence of the sympathetic, for he defines it, "a nervous system which exists in the lowest scale of animals, is peculiar to the nutrient organs, and performs its functions through an especial power, which antecedently to the appearance of the brain constitutes a nervous centre, and which, when the cerebral centre exists in animals, is connected with it; but never composes with it one individual apparatus." He places in its centre all the sensations perceived at the epigastrium, a centre which Wrisberg has denominated the abdominal brain.

Without pretending to express the opinion that Bichat, Reil and their followers have succeeded in establishing their theory beyond the possibility of doubt and cavil, we believe that it may in many respects be regarded as preferable to any other with which we are acquainted,—as meriting at least serious attention, in the explanation of physiological and pathological phenomena; and as serving to dissipate the obscurity in which several of the operations of the economy had been involved. We cannot help noticing here, that the independence of action of the sympathetic was a necessary provision of nature; since this nerve supplies parts intrusted with functions, which in order to the preservation of life must be continued without any interruption—an advantage that could not have been obtained, had the nerve presiding over the organs intrusted with these functions been subjected to all the vicissitudes of impressions to which the nerves of animal life are constantly liable, and to the influence of volition. This indeed would alone suffice to establish a line of demarcation between the two nervous systems (continuance of action in the one, intermissions in the other), and render very probable the theory of Bichat respecting the two lives, animal and organic, and the agency of the two sets of nerves over them. But more of this presently.

Mr Charles Bell, who, as is well known, has greatly distinguished himself within a few years by his researches into the functions of the nerves, but who, having found nerves of sensation and nerves of motion forming together a nervous

circle, does not appear to know what to do with the visceral nerves, combats the opinion of Bichat respecting the functions of the ganglia and sympathetic nerve, on the ground that the name of ganglionic is not correct, since thirty-one pairs of large ganglions in regular order and carefully protected, like important organs, are to be found in the nerves of the head and spine;—that there was no discovery on the part of Bichat, since others had already shown that the sixth nerve was not the root of the sympathetic, and the opinion of its independence of the brain had already been propagated; that the nerves which are found in the lower animals, and give them the power of sensation and volition, (attributes which Bichat denies to the sympathetic,) are not, as maintained by this author, the rudiments of the ganglionic system in man. (See his Lectures delivered at the College of Surgeons in 1828, Lond. Med. Gazette, vol. i. p. 556, &c.)

From this and from having nerves of motion and sensation, either regular or superadded, to account, as he thinks, for all the functions, Mr Bell concludes that we know nothing of the use of these nerves; and that all that has been said on the subject is erroneous. His language is worth quoting: "you know," (he addresses his class,) "that they have long been called the sympathetic nerves, because all the connexions of the parts, whether in the performance of their natural functions, or exhibiting the symptoms of disease, were accounted for through these nerves; even when you sneezed, the consent of parts was explained by the relations established through this extended nerve. Not one word of this is true. The sympathetic system of nerves ministers neither to sensibility nor motion of any kind: they have nothing to do with the operations of the sensorium, and all the actions of the respiratory organs are independent of them. They are known to us only by their negative qualities, and we are left to conjecture their uses; that they may serve to constitute the living body a whole, forming that bond which unites the several viscera together; making one dependent upon the other, forming them into a circle; uniting the body, the head, and limbs, so as to be one in constitution; they may be subservient to circulation, secretion and absorption. I can see no objection to the term sympathetic system; or, if you choose, the consti-

tutional; as they are the means through which the constitutional powers are exercised, independently of sensibility or motion." Mr Bell has repeated the same sentiments on this nerve, nearly in the same language, in the introduction to his work, on "the Nervous System of the Human Body," published in 1830, and adds that "constitutional peculiarities, fever, and general derangements of health must belong to this system of nerves."

By those who are acquainted with the labours of Mr Bell, it will be admitted, that he has contributed, in a considerable degree, to the advancement of our knowledge relative to the functions of the nerves; that he has even made some useful discoveries in this department, and thereby helped to dissipate a portion of the obscurity, in which many operations of the economy were previously involved. But we cannot be made to admit that he has said on the subject of the nervous system *all* that ought to, or might, be said upon it; that he has brought the subject to a degree of certainty equal to a mathematical demonstration; in a word, that he has settled the matter beyond the possibility of dispute. We know, indeed, that his opinions have not received the sanction of all his contemporaries, and that many of his views respecting the functions of some of his superadded nerves, about which he and his friends have said so much, are stubbornly opposed by physiologists of high rank in and out of England. This, however, could have been foreseen, for Mr Bell can have no more pretensions to infallibility than he ought to have, if we were to judge only from the specimen before us, to consistency, logic, clearness of thought, or accuracy of language. Let us remark, en passant, that the sympathetic has proved a kind of stumbling block to the two great physiological rivals of England and France, and that on the subject of this nerve they have been led, not from a more careful analysis of physiological and pathological phenomena, and from the results of experiments, upon which the doctrine of the functions of the sympathetic could alone be established; but from the particular views they have adopted respecting the functions of the cerebro-spinal nerves, to advance strange things, and to come to conclusions more or less at variance with established facts and received opinions. Of the idea of one of those

writers relatively to the *non nervous* nature of the ganglionic system, and of the inconsistencies into which this idea has led him, we have already spoken somewhat at large. As regards the English Haller, we will confine ourselves to a few remarks. Had Mr Bell simply said "I know nothing of the uses of these nerves," we should not have pretended to find fault with him for so doing, for we are ready to maintain that every one has a right to avow his ignorance on any subject, without being amenable to the censure of a reviewer. Considering the way the world runs now a-days, we would even give an author credit for the unusual degree of frankness such a conduct would indicate. But Mr B. has made use of the word *we*, by which he indicates every one—a general charge of ignorance on his contemporaries, which all of them may not be pleased to hear, and which calls for a few remarks on the principle of self defence.

It will be seen from what precedes, that Mr Bell offers not one word of argument to disprove the views of Bichat, and that his objections to the term ganglionic and to the claims of priority are trifling and unworthy of him. We may also remind our readers, that some of the assertions, by which he tries to combat the French physiologist, are disproved by facts we have already stated, and which are recorded by writers of equal authority in the science with himself; and that on the subject of the sympathetic we have merely his *ipse dixit*, unsupported by a shadow of proof, and rendered still less admissible from its being obscured by the most glaring inconsistencies. Thus we are told, that the sympathetic is not destined to fulfil the uses which former physiologists have pointed out—that we know nothing about its uses; yet in the same page it is allowed that these functions may consist in something which he has specified, and this something is precisely what was contended for by other writers! We are told that the sympathetic system neither ministers to sensibility nor motion of any kind, and yet that it may be the bond of union which unites the several viscera together,—an office it could hardly fulfil unless it possessed sensibility of some sort or other; that it has nothing to do with the operations of the sensorium, and yet forms all the viscera into one circle, and unites the body, the head and limbs; that these nerves may be subservient to circulation, secretion and absorption, and all that independently of sensibility or mo-

tion; that constitutional peculiarities, among which are strangely enough placed *fever* and general derangements of health, belong to this system of nerves, although these are destitute of the usual characteristic properties of nerves.

For us a cord which ministers neither to sensibility nor motion is not a nerve and cannot link parts together nor influence any function. It hence follows, that all Mr Bell has said on the subject of the sympathetic merely proves, at least to our minds, that this nerve was in his way, and embarrassed him in his beautiful theory relative to the functions of other branches, regular or superadded. But this is not all: in the same lecture and in the same introductory chapter, Mr Bell censures Bichat for admitting the existence of two nervous centres; that of animal life consisting in the brain, and that of organic life in the ganglia; and remarks that it is anatomically incorrect, since the spinal nerves have ganglia. Yet at page 12 of his large work it is admitted that "the semi-lunar ganglion and the solar plexus being parts of this system, and the branches of nerves extending and diminishing from this region, give countenance to the idea that we have here the centre of the sympathetic system," an idea "countenanced by the fact, that these viscera perform functions the most independent of the will and over which the mind has no control." It follows, from the extracts, on which we have commented, that even Mr Bell is forced to acknowledge, that the sympathetic presides over some of the involuntary functions, and is the agent of the organic sympathies; and that as this nerve is independent of the sensorium, and endowed with a peculiar sensibility, (for that it possesses some *must* be allowed, though he has denied it,) the theory of Bichat and Reil is not as destitute of foundation as he chooses to affirm.

The question at present before us may, we think, be stated and decided in the following manner. The sympathetic, considered as a whole, is nervous; therefore it generates or transmits nervous influence. It is different from other nerves in its organization, and in its mode of acting when placed under the influence of stimuli. Its mode of life then is different from that of the other nerves; consequently it must furnish a nervous force different from that supplied by the other nerves. It supplies parts having few if any other nerves; consequently those parts

are under a nervous influence different from that by which other parts are animated. These parts are entrusted with the performance of peculiar functions, and as all functions require for their performance nervous action, it follows, that the parts to which we allude must derive the peculiarity of their functions, not only from the peculiarity of their organization, but from the quality of the nervous action imparted to them; finally, the functions of the parts in question are not under the *direct* control of the cerebro-spinal centre, therefore it is probable that the system from which they derive their nervous energy is itself not under the *direct* control of that common centre.

It will be unnecessary to undertake an elaborate enumeration of all the circumstances that might be brought forward in confirmation of the doctrine of Bichat, relative to the sole agency of the sympathetic system in the functions of nutritive life, and, in consequence, to the isolation of these from the life of relation. Such an attempt would force us to extend this article beyond our prescribed limits, lead us into many and useless repetitions, and make us anticipate what we have to say on the subject when on the functions separately. Besides, it will be easily seen, that all the facts and arguments we offered when speaking of the functions of the ganglia—particularly the fact of fœtal life continuing, although the child be deprived of brain and spinal marrow—will apply to the functions of the sympathetic, considered as a whole; whether we admit this system to be completely independent, or only so to a certain degree; for in the latter case the nervous energy or power derived from the spinal marrow must, before being transmitted to the viscera and becoming subservient to their functions, undergo a great modification in the ganglionic system, and be mixed up with, or reinforced by, that evolved in the latter.

Although we are disposed to contend with Bichat and other writers of the same school, that the nervous system of organic life—the great sympathetic, is independent of the other system of nerves, and of the centre of volition, owing to its being itself a focus of innervation, or to the ganglia breaking the force of, and modifying the nervous action passing from the spinal marrow to the ganglia and plexuses, and vice versa, we acknowledge, as we have already stated, what in fact every one

must acknowledge, that there exists a communication between the two systems, without which many of the phenomena observed in health, as well as in a diseased state of the system, could not admit of a ready explanation. Thus the branches of the sympathetic penetrate with the arteries into all the tissues, and must necessarily intermix with the cerebral and spinal nerves which supply these parts. This is particularly shown to be the case in the larynx, pharynx, bronchiæ, lungs, stomach and genital organs, where the filaments of the sympathetic mix with the eighth, ninth and lumbar nerves. It unites, besides, by means of small filaments with all the spinal ganglions, and passes, together with the arteries, into the substance of the brain and spinal marrow. Nature seems, indeed, to have been careful in multiplying these anatomical connexions of the visceral nerves with the cerebro-spinal centres and cords, in order to ensure the reciprocal influence of these parts, and thus guard against their *perfect* isolation, which, had it existed, would have prevented the establishment of that harmony of action, so requisite between all parts of the body, and have proved very soon fatal to the individual.

In disease some of the acts of life being set out in bolder relief than during the normal play of the functions, this reciprocal influence becomes much more conspicuous—for example, irritations of the splanchnic nerves occasion effects on the brain and spinal marrow, which, reacting and producing a reversion of stimulation on the muscular apparatus of animal life, give rise to partial or general convulsions. On the other hand, inflammatory or simple irritations of the brain, intense passions of the mind, which may be called *disease*, derange often the functions of the heart, stomach, intestines and secretory organs. In such instances the ganglia have not offered a sufficiently powerful barrier to the stimulation, which consequently has traversed them and spread to the other system of nerves. On no other principle are we able to explain, in cases of acute gastro-intestinal or hepatic irritation, the symptoms of cerebral or cerebro-spinal irritation which supervene in the course of the disease; and in chronic cases, the phenomena of hypochondriasis which frequently result from nervous and slow inflammatory affections of the viscera; the effect of worms in the intestines in producing nervous complaints;

apoplexy, and other cerebral disease from the use of indigestible food or chronic gastro-enteritis. A thousand other examples of the same kind might be cited; but the preceding will be sufficient for the present.

But in such cases,—even in those in which this reciprocity of influence between the two portions of the nervous system is most manifest,—whether in health, for the exercise of a function, or in a diseased condition of the system, the effects produced are placed beyond the controul of the will. Every one knows that a passion of the mind will occasion palpitation of the heart—a superabundant secretion of bile or of urine,—vomiting,—intestinal evacuations, &c., but it is impossible by the simple operation of volition to cause these phenomena.

We are well aware that against the view of the nature and functions of the sympathetic which we here advocate, it may be urged, that when we investigate the phenomena which take place in the lower order of living beings, we discover that impressionability and motility under the influence of the excitation of fluids, vital affinity, or the chemical forces properly so called—in other words, the power of attraction existing in solid organic molecules, or in the fluids of the body, as well as the inherent action of the cellular and vascular tissues, derived from their proper irritability, would, strictly speaking, appear to suffice for the accomplishment of the phenomena of life, without the concurrence of a system analogous to the nervous system;—that plants, as we are told, have a real circulatory movement, a respiratory oxygenation, exhalations, absorptions, secretions, nutritions, assimilations, in a word, a primary mode of existence, although nothing similar to nerves or ganglia have been discovered in them;—and that from this it has been inferred, by some physiologists, that the ganglia and nerves are not indispensably necessary to the accomplishment of the essential phenomena of nutritive life,—to the fundamental acts of circulation, respiration, secretion, and assimilation.

But we are not sure that these inferences are correct; 1. Because it is not proved that plants are destitute of nervous matter. M. Dutrochet, as our readers will find by referring to the sixth volume of this Journal, M. Brachet and others, contend for the existence of nervous matter in vegetables; the latter endeavouring to show that every living body possesses

at least a ganglionic system or its equivalent; and 2. Because, supposing that plants are really deprived of nerves, we are not prepared to regard all those fundamental operations of life in the two classes of living bodies—plants and animals—as precisely the same, and produced by exactly the same mechanism. But even granting that the facts assumed, and the inferences drawn from them, are correct, might we not be allowed to look to other circumstances for forming an idea of the functions of the nervous system, and particularly of that portion of it which has received the denomination of organic? This requires explanation.

Physiologists have very properly remarked, that while the plant receives all the materials of its growth and ultimate development from the soil in which its germs have been deposited; that while in it local action alone is necessary for the prehension of these germs; the animal, on the contrary, is forced to move in order to seize the objects required for the gratification of his wants. Some of the phenomena of life, nutrition for example, may, it is true, still be independent in their essence of the nervous system; but their preparatory acts appear in animals to be submitted to peculiar determinations, and the life of the individual is, in a great measure, submitted to the controul of his own arbitrary will.

Moreover, the transportation,—the conveyance of the products of the secretions, and of the nutritive materials, differ essentially in the two classes; the phenomena of the excretions are more complicated in the animal than in the plant. Circulation in the former is not limited to that of white or lymphatic fluids, which alone can be compared to that of the sap in the latter; it has for its object the transmission to all parts of the system of a distinct fluid from which the materials of nutrition are derived. It is no longer a simple movement of transportation, effected by the force of capiliarity and motility of vessels excited simply by the presence or contact of the fluids. So far from this, it requires for its accomplishment the agency of a central organ, and of a particular set of vessels, by which an impulse is given to the fluids,—an impulse, indeed, requiring a higher degree of energy in the exercise of the power of motility, which, in its turn, cannot be brought into play without a more powerful cause of excitation than

could be furnished by the simple action of the fluids on the irritability inherent in the organ. In animals also the faculty of receiving impressions, of being apprised of the contact of various external bodies, in order to select those that are necessary, and reject all such as are detrimental to the system; the power of reacting against these impressions, of transferring their effects to parts distant from those in which they were received, of bringing into play various and distant parts for the accomplishment of certain ends, of uniting into a homogeneous whole, the various elements of the body;—the power, in the more elevated classes, of reflecting on the wants of the system, and exerting the will and judgment in the execution of the movements necessary for their gratification,—all these become requisite for the full accomplishment of the phenomena of life. From all these circumstances arise the necessity of new means and new forces in the economy, and as a natural consequence, of a distinct organic apparatus. The animal, in fact, is endowed with a distinct mode of life which renders certain acts that, though of a secondary kind, are not less indispensable to the maintenance of his existence, necessary to the performance of the functions;—which acts have required the introduction of an additional apparatus. This apparatus, which varies in complexity in the various classes, according to the complexity of their organization, has received from anatomists the name of *nervous*.

Without stopping to inquire into the organic structure or the topographical anatomy of this system in the various classes of animals—without examining whether, in the lower orders it is homogeneous in its organization and general in its influence over all the functions of the body,—subjects which would be foreign from our present object, it will be sufficient to remark, that in man, and in other animals of the superior classes, the nervous system is complex in its organization and functions; that some of the acts of the economy appear to be entrusted to a particular portion of that system, the centres of which are the brain and spinal marrow;—that these place those acts more or less under the influence of the will, and endow the animal with perceptible sensations. But there are other acts upon which these centres and their cords exert only an indirect, if any, influence,—over which volition

has no empire,—the organs entrusted with the performance of which are supplied principally by filaments from another set of nerves,—having but an indirect communication with the centres of the former set, is somewhat peculiar in its organization and textural properties, and of which, until a comparatively short time, nothing very definite was known. From the difference existing between these acts and those submitted to the power of volition; from the absence of nerves of the cerebro-spinal system in organs entrusted with their performance, and their supply from the other set, it was natural to refer those acts to the agency of the latter; admitting, however, that for the harmony, so requisite between all the functions of the economy,—without which all individuality would cease, and a disunion of action in the different members of a whole would ensue and prove detrimental to the regular play of life, a communication, an accord, is established by means of those filaments which, as we have seen, while speaking of the topographical anatomy and origin of this nerve, (we allude of course to the sympathetic), it sends off to join the spinal nerves and the sensorial organs. This is acting agreeably to the method of exclusion; the first step being to show, that the cerebro-spinal centres and their nerves have certain specific functions to perform; that they exercise only a secondary influence over the organs of internal life and the functions of nutrition, circulation, &c.; that as these organs receive nerves from a different system, having but a secondary communication with the cerebro-spinal system, and which is the principal focus of its own vital power, they must be under the especial influence of those nerves. This method, when combined with the experimental method and with the proofs derived from anatomy and pathology, must, we should suppose, be sufficient to render the views of Bichat and Reil highly plausible.

It may be urged, also, that in the lower orders of animals the functions of nutrition are performed, though the ganglia and their cords are shown to belong to the spinal system of nerves. We admit that this objection appears to have some weight; and we have even gone so far as to maintain, that in the present conflicting state of opinion respecting the nature of these ganglia and filaments, the argument founded on their presence in lower animals, by Bichat and his followers, should be abandoned.

But if we have agreed to give up that point on account of the difficulty of proving the nervous system in question to be analogous to the ganglionic of the higher orders of animals, we think it is but fair that those who advocate a different view of the subject, should also cease to adduce the presence of these ganglia and filaments, which they regard as of a different nature from the sympathetic system, as proofs that this system is not the one charged with the functions of organic life. 1. Because they have not proved that these nerves do *not* belong to the sympathetic system, any more than others have proved that they *do* belong to it. 2. Because in the animals occupying the lowest grade in the scale of animalization, the functions are so simple, that the same system of nervous globules may be supposed adequate to preside over them all. 3. Because in some of a higher order nothing but filaments without centres are found, and it would be difficult to decide whether some of these do not belong to a different class of nerves from the others. 4. Because in those in which ganglia are found we cannot be certain that, even admitting these to belong to the spinal system, some nervous matter of a different kind does not exist, though it cannot be distinguished from the other.

It has been urged by those physiologists who advocate the spinal origin of the sympathetic as a peculiarity of the organs supplied with its filaments, that they derive their power of action from (we would say, are influenced by) every part of the spinal marrow; whereas, other portions of the body are only animated by that point of the cord from which they receive their nerves. This circumstance, we believe, was first pointed out by Le Gallois, who explained by it the intensity of the power of the former, (but particularly of the heart, to which he had reference in his experiments;) the uninterrupted continuance of their action from the time of conception to that of death, and their independence of volition; while the functions of the latter are under the controul of the will, are temporary, and can only be renewed after complete intermissions of greater or less duration. This distinction, as will be readily seen, divides very nearly the same organs as those referred by Bichat to one or other of his lives, but evidently rests upon a very different basis from that assumed by the latter, since the organs included in

the organic life, which in Bichat's doctrine are considered as independent of the brain and spinal marrow, are precisely those which, according to Le Gallois, receive from them the most powerful influence—the only difference of these results from those of ordinary effects of volition depending on the vastness of the origin of the nerves of involuntary motion, and the requisite influence of the whole spinal centre. The opinion of M. Le Gallois was founded upon the results of his experiments on the spinal marrow; the action of organs supplied with spinal nerves being arrested by the destruction of that part of the cord from which these nerves originated; whereas, an interruption to the functions of organs supplied with ganglionic nerves was only obtained when the whole marrow was destroyed.

The experiments of Dr Wilson Philip led to conclusions somewhat different from those of Le Gallois. After maintaining that the powers of the heart, of the vessels of circulation and of secretion, and of the peristaltic motion of the stomach and intestines, are all independent both of the brain and spinal marrow, and to be referred to their irritability or to the mechanism of the muscular fibre itself; he adds, that these organs, as well as the voluntary muscles, are capable of being influenced through the nervous system, (Inquiry, 257); that the ganglions are secondary centres of nervous influence, whose nerves are as extensively distributed as those which proceed from the brain and spinal marrow, (Inquiry, 257, 264,) and that the ganglions are the means by which the influence of every part of the brain and spinal marrow is bestowed on the parts which we find influenced by every part of these organs. At page 264 of his Inquiry he states, that the two sets of muscles of voluntary and involuntary motion "are equally capable of being excited by the nervous influence; but while this influence is the sole stimulus to which the muscles of voluntary motion are subject, it acts only occasionally on the muscles of involuntary motion, which are excited in all their usual actions of stimuli independent of it, and consequently of the will. When the latter muscles are excited by the nervous influence, it is not applied to them in the same way as to the muscles of voluntary motion, to which it is sent directly from the brain and spinal marrow, each muscle receiving the

nervous influence from a particular part of these organs; while to the muscles of involuntary motion, it is sent through the great chain of ganglions, each muscle receiving its nervous influence from every part of the brain and spinal marrow. In a paper subsequently read before the Royal Society, and published in the Philosophical Transactions, in the London Medical Gazette (vol. v, p. 487, &c.), and for a third time in his late work on acute and chronic diseases, Dr Philip has presented his views respecting the functions of the ganglionic nerves in a condensed form. He concludes from all his experiments, that the ganglionic system is destined to establish a general communication between the parts of the body it supplies with nerves and *all* the others, as well as to favour the reception, by those parts, of stimuli, from all parts of the brain and spinal marrow;—a circumstance which, as we have seen, does not hold in reference to parts supplied with cerebro-spinal nerves, as these receive the influence of only one portion of the spinal marrow or brain, and convey their impressions to only one portion of the body. This circumstance he attributes to the perpetual intertwining of the nerves of that system, effected not only by its ganglia and plexuses, but also by the very free anastomoses of its filaments.

Dr P. very properly remarks, that the partial connexion of the organs supplied by cerebral and spinal nerves, with the nervous system, and the universal connexion with that system of those supplied by the ganglionic nerves, explain many of the phenomena, both of health and disease. Why, he asks, are affections of the stomach and other important organs felt instantly through every part of the frame, while those of a voluntary muscle, or even of an organ of sense, although often possessed of great sensibility, is confined to the injured part? If the eye or ear, or the muscle of a limb, be so deranged, by a sudden blow, for example, as instantly to destroy its power, the senses of sight, or hearing, or the voluntary power of the part is lost, and there the evil ends, unless inflammation ensues; but a blow on the stomach, which instantly destroys its power, at the same moment destroys that of every other part. It is not difficult to answer the question, since the state of the stomach, from the cause just pointed out, may influence every part of the nervous system.

These remarks, and the experiments which have elicited them, are certainly highly interesting in a physiological and pathological point of view; but we must confess, that they do not appear to us to justify the conclusion to which the two distinguished physiologists we have named have arrived. All, it appears to us, that we can fairly gather from Le Gallois and Philip's experiments is, that the brain and spinal marrow, throughout their whole extent, exert an influence over the vital organs. To this opinion we are led by the consideration of some facts which may be collected in the writings of Dr Philip himself, and of other experimenters, as well as by the nature and results of some of Le Gallois's experiments. If the vital organs really received their nervous power exclusively from the spinal marrow, as contended for by Le Gallois, we should find that this power would be annihilated by the destruction of that system; and such, indeed, is precisely what Le Gallois maintains on the strength of his experiments. But independently of the facts already mentioned, and which might be cited to exemplify the continuance of action in vital organs, although no influence from the spinal marrow could be supposed to be furnished,—we allude to anencephalus fœtuses, we may allow some importance to the circumstance, that in M. Le Gallois's experiments the sudden destruction of the spinal marrow must have occasioned, in consequence of the disruption of that reciprocal influence which the two nervous systems furnish to each other, and which we have said is necessary to the free operations of life, so violent a shock to the functions of the vital organs, as to occasion the sudden death of the animal. In the case of fœtuses without brain or spinal marrow, on the contrary, the destruction takes place slowly, and the vital organs become so gradually accustomed to the change as not to be affected by it. Do we not see this, every day, in various forms of disease? Does not a sudden and violent morbid impression on the spinal marrow or brain occasion a correspondingly violent effect on the vital organs,—pectoral and abdominal; while, in chronic diseases of those parts, even when these diseases have occasioned disorganization, little or no change in the rhythm of the vital functions is produced? Do we not find this difference of effects in the influence of other organs on each other, when suddenly or slowly affected? Le Gallois himself, as Professor Lallemand judiciously remarks,

has found that by destroying the spinal marrow slowly, and by small portions at a time, and by allowing some time to intervene between each operation, he could destroy a greater mass of it without producing death, than when he destroyed the whole of it suddenly. But independently of this, the spinal ganglia—the tract of nervous matter which gives rise to the nerves of sensation—that from which the nerves of voluntary motion arise, and the lateral columns connected with the office of respiration must all have been destroyed (Charles Bell). To this must be added that M. Le Gallois and Dr Philip found that the cervical portion of the spinal marrow, the whole of that organ, and even the brain itself, might be destroyed, without apparently affecting the action of the heart, provided artificial respiration were maintained—that Mr Clift states, that in his experiments on the carp, the action of the heart continued long after the brain and spinal marrow had been destroyed, and that Dr Philip found the capillary circulation and the muscular action of the stomach and intestines to continue unimpaired for some time, the former in a frog's web, the latter in a rabbit, after the destruction of the spinal marrow or brain, or both conjoined.

A due consideration of all these facts and arguments will serve to convince us, that the ganglionic system, though influenced in some measure by all parts of the cerebro-spinal system, is yet, for all that regards its agency over the regular functions of the vital organ, and particularly in the fœtal state, when the harmony of action between the two portions of the nervous system does not exist, and is not in fact necessary, independent of the cerebro-spinal centres; and consequently, that when the influence of the latter is exerted, it must be regarded as something additional,—superadded and, strictly speaking, unnecessary to the regular play of the vital functions.

This article has reached already to such an unexpected extent, that we are forced to stop somewhat abruptly, and to leave unnoticed many points connected with the physiology of the ganglionic system. In some future number we may find an opportunity to resume the subject, and investigate the agency of the sympathetic in the production or maintenance of the vital functions separately,—circulation, respiration,

nutrition; in the production of the sympathies; its influence over the exercise of the sensorial faculties; in the production of animal heat, &c. On many of these points much of interest may be said, and much will be found contained in Professor Lobstein's and M. Brachet's valuable works.

In termination, we may be allowed to hazard a few suggestions: 1. If it be true that the sympathetic must be considered as a distinct and independent system of nerves, we naturally conclude that it possesses a *centre*, as the nerves of animal life possess one in the brain and spinal marrow; for it would in all probability be going too far to admit with Bichat that *each* ganglion must be viewed in that light. But even in admitting that each ganglion serves as a focus of nervous power, and a receptacle of sensations of its appropriate organ, yet we must suppose that it maintains its connexion with, and its dependence upon, the rest of the same system. Dr Miller has well remarked, that "one ganglionic circle may be thrown into commotion by the presence of unaccustomed objects, without imparting either its knowledge or disorder to any other." This is exemplified by the action of a cause interrupting the functional action of an organ, without immediately or materially affecting others. The same results take place in experiments on the ganglia. But it is not less true that the affection of a ganglion, whether produced by a common cause or by the experimentalist, will soon occasion disorder in all the others.

2. If each ganglion cannot be considered as a distinct and independent centre :—if there is a common centre to the sympathetic system, where are we to look for it? If we may be allowed an opinion on the subject, we would say, that the great mass of ganglia and nervous anastomoses, denominated semilunar ganglia and solar plexus, should be considered as at least the principal centre, and that the other ganglia found in different parts should be held only as subsidiary centres. Our reasons for this opinion are founded on the facts mentioned above, and which, as we have seen, induced Mr Bell to adopt the same views on the subject. They need not, therefore, be recapitulated here. By a reference to the 5th volume of this Journal, p. 435, our readers will perceive that M. Jules Arthaud, of Paris, struck with the results of the dissection of the spleen,

performed in 1817 by Mr Strauz, and in which this naturalist found that organ filled with a pulpy substance in all respects similar to the nervous pulp, and following up the idea of the same gentleman, that "the spleen was a nervous plexus, dependent upon the ganglionic system of the viscera," that M. A. we say, instituted a series of experiments, from the results of which he has come to conclusions very similar to the foregoing. In the 6th volume of the same work it will further be seen, that Dr Miller, of Baltimore, likewise considers the spleen as the "source or centre, or great secreting gland" of the ganglionic system. Upon these views we have already expressed an opinion, and do not intend, therefore, to enlarge in this place. We merely refer, for their development, to the volumes already mentioned, and to Dr Miller's late publication.

3. As the ganglionic system has a centre, it must follow, that, whenever a sensation is received by one or more twigs of this system, the impression is necessarily transmitted to that centre before any general effect can be obtained. Now if, in consequence of this sensation, the cerebro-spinal centres become in any way implicated, experience pain, or are called upon for certain acts either normal or abnormal—this effect can have been produced in only one of the three following ways: 1st, the nerves of organic life, in which irritation has been reflected by the ganglionic centre, have communicated it to the nerves of animal life, with which they anastomose in the organs, and which, in their turn, have conveyed it to the brain. 2d, the stimulation has been communicated directly to the cerebro-spinal centres by the ganglionic nerves, which, as we have seen, dip into these centres, either along with the arteries or with the nerves of sensation, and there convey the commands of the ganglionic centre. 3d, the irritation is received in the organic centre itself by nerves of animal life, and is transmitted by these to their own centres.

4. It is plain that the reverse may take place through the same agencies, when the impression is transmitted *to*, instead of *from*, the ganglionic centre. In that case the impression must be conveyed to the centre of the ganglia by the sympathetic nerves themselves, or by branches of the system of animal life. This latter is easily accounted for in the upper

cavities, or in the upper part of the abdominal cavity. But in the lower portion of the latter how are we to explain this transmission? It cannot be effected by the branches connecting the ganglia of these parts with the spinal marrow, and which we have described, since these, in all probability, go *to*, and do not come *from*, this medulla; besides, they join cords which are principally composed of nerves of sensation. To account for the phenomenon we must have recourse to the common ganglionic centre, which, like the cerebro-spinal centres, becomes the reservoir of all impressions transmitted from these centres, and reflects them to various parts under its dependence; or else, if we must admit a direct agency of the spinal marrow on vital organs which do not receive nerves of animal life coming from the upper part of that centre, we are forced to suppose that it furnishes twigs which have as yet escaped the researches of anatomists.

5. It is probable that the nerves of the sympathetic, like the nerves of animal life, are of the two orders,—of sensation and of motion, since movements of the organic kind are produced in parts unprovided, at least so far as we can ascertain, with cerebral or spinal nerves, or which do not appear to require the intervention of the brain or spinal marrow, and of course of the nerves that issue from or proceed to these,—which, indeed, are influenced only in a secondary manner by their diseases or their destruction, and have been known to occur in cases in which these centres did not exist. This idea has been defended with considerable ability by M. Guérin de Mamers in France, and by Dr Miller of Baltimore.*

* Dr Miller is Professor of the Institutes and Practice of Medicine in the Washington Medical College at Baltimore. He has favoured the profession with his views on the subject of the sympathetic, and on other points, in several appendices and notes inserted in a reprint of Dr Philip's recent work on acute and chronic diseases, which appeared lately at Baltimore. While we take pleasure in acknowledging our obligations to a talented countryman for contributing his share towards the elucidation of some of the most obscure points of physiology, we cannot help expressing the regret we experienced in perceiving the tone of confidence with which the claim to originality respecting those views has been made by Dr Miller. In his preface, the doctor announces that he owes nothing to Bichat, Hewson, Home, &c. &c. except for the corroboration of his previous conceptions—a fact which, he asserts, could be proved by the order of time;—nay, he goes so far as to maintain that his *views are so entirely different from*

Art. XI.—GANGRENE OF THE MOUTHS OF CHILDREN.

Der Wasserkrebs der Kinder; von Dr A. L. Richter, *Arzt des Fredrich-Wilhelm-Med. Chirurg. Instituts. Staabs-arzt, &c.* 8vo. *Berlin,* 1828.

Beobachtungen und Bemerkungen über den Wasserkrebs; vom Dr C. C. Hueter, *Gehülfsarzte bei der Chirurgischen abtheilung des Land Krankenhauses zu Marburg.* 8vo. *Berlin,* 1829.

By the term wasserkrebs, or aqueous cancer, the German writers designate that peculiar affection of the mouths of children, terminating rapidly in extensive sloughing of the gums and cheeks, and not unfrequently in necrosis, also, of a considerable portion of the maxillary bones. The attention of American physicians was first especially directed to this disease by Dr Benjamin H. Coates, in a paper published in the third number, volume second, of this Journal, presenting a very interesting account of the gangrenous ulcer of the mouth, as it occurred in the Children's Asylum of Phi-

those of all others in their most marked features, that he trusts he will not be taxed *with dependency upon any extrinsic aid.* With respect to that part of the claim to originality which relates to the great sympathetic, on which we are inclined to think Dr Miller attaches the greatest importance, we must be allowed to remark, that it is totally unfounded, since his views, and almost all the details and arguments upon which they are founded, will be found contained in Bichat's General Anatomy. Dr Miller remarks, that "the innovations in the principles of medicine, to which he lays claim, are the results of experiments, observations, and reflection, begun more than twenty years ago." Now, Bichat's work was published in 1801, consequently 30 years ago, and ten before Dr Miller made his discoveries, or at least began the experiments, &c. which led to those discoveries. If to this it is added, that Bichat's reputation certainly entitled him twenty years ago to a careful perusal from all who aspired to the rank of an innovator;—that in fact he was not the first, as we have shown, to regard the ganglionic as an independent system of nerves, and the ganglia as distinct centres of nervous action, we shall be able to arrive at a just estimate of Dr Miller's pretensions to originality. Dr Miller would have acted more wisely had he reprinted Bichat's chapter on the subject, and added a few notes in explanation of his own views on points in which he differs from the French physiologist.

As regards his notions respecting the nature and functions of the spleen, we have stated in the text that he has been anticipated on the subject some years

ladelphia, and the treatment which was there found most effectual in arresting its progress.

The gangrene of the mouth, when we consider the insidious manner of its attack, the rapidity of its progress, and its very great mortality, when neglected or mismanaged, must be ranked among the most formidable of those diseases which occur during the period of infancy. It is one, the pathology of which demands a much closer and more extended investigation than it has yet received.

The disease is happily one of comparatively unfrequent occurrence in this country. In Europe, however, it would appear to prevail to a very considerable extent, especially in the hospitals destined for the reception of infants, among the children of the poorer classes, and in low, damp, and otherwise unhealthy districts. According to Thomassen and Thyssen, two Dutch writers, who have recently published accounts of the gangrene of the mouth of children, it occurs frequently as an epidemic in the Netherlands, after various eruptive and gastric diseases.

The first accurate account we have of this affection is that of Battus, contained in his Manual of Surgery, published at Amsterdam, in 1620. He denominates it *water-kanker*. Subsequently mention is made of the disease by many of the older medical writers of Holland, France and Germany, and it is described with some minuteness in the more recent treatises of Hildenbrand, Girtanner, Fleisch, Feiler, Henke Jörg, and Reimann, on the diseases of children. But the facts

ago by several French physiologists. Here again, therefore, his claims to originality is unfounded.

We cannot terminate this note without alluding to one more point connected with Dr Miller's views on the subject of the sympathetic. Dr M. states, that "so far from the spinal nerves sending branches to the sympathetic, those two kinds of nerves, in the intercostal course, do not unite." This is incorrect, for we have always found, even since reading the above lines, that the sympathetic *does* unite with the intercostal nerves, and becomes, so far as we can perceive, confounded with them. This is a point on which, indeed, the most respectable anatomists agree.

We are happy to state that Manec's splendid lithographic plate of the sympathetic has been copied by Mr Ancora, a distinguished drawing master of this city, and published by Mr Dobson, No. 108, Chestnut Street. We regard it as the finest anatomical plate of the kind we have ever seen, and as such recommend it to all who wish to form a correct idea of the anatomy of this nerve.

which these writers present in relation to the disease, are so few and imperfect, and in many respects so contradictory, that nothing certain can be deduced from them in relation to its actual pathology. Notwithstanding the gangrene of the mouth of children is a disease of very common occurrence in different parts of England and Ireland, it has elicited but little attention from the physicians of these countries. With the exception of the imperfect notices contained in the works of Boot, Simmonds, Pearson, Underwood, Parr, Burns, and S. Cooper, there is scarcely a medical writer of Great Britain by whom any mention whatever is made of the disease. We must except, however, that variety of it denominated by Dr Jackson, of Northumberland county, Pennsylvania, *gangrænopsis**, which is very accurately described by Mr Dease, of Dublin, in his treatise on the diseases of lying-in women.

Recently, however, the attention of the French and German writers, particularly the latter, has been directed to the disease under consideration, and we may shortly expect, as the result of their labours, a more accurate knowledge of its nature and treatment than we now possess. Within a few years four dissertations on the gangrene of the mouth have been published in Germany by Drs Z. Reiman, W. J. Weigand, A. L. Richter, and C. C. Hueter. By the first of these gentlemen the term cancer is very properly rejected; he proposes to substitute the denomination, "*highly acute pulpy or caseous gangrene.*" The treatise of Dr Richter, the title of which is placed at the head of this article, presents a very excellent view of the present state of our knowledge in regard to the causes, symptoms, pathology and treatment of the disease; we propose, therefore, to lay before our readers a very full analysis of the most interesting portions of its contents, with the addition of such remarks as may suggest themselves from the result of our own experience.

The opinions of Dr Hueter will also be noticed in the course of the review.

According to Dr Richter, the general symptoms by which the gangrene of the mouth is accompanied, have a close resemblance to those proper to the disease described by the

* See Medical Recorder, Vol. XII. page 66.

older writers, under the name of *stomacace*, of which he conceives the present disease to be in fact a mere variety. The affection of the mouth is preceded, for a longer or shorter time, by a series of symptoms more or less numerous, and differing in some degree according to the cause by which it has been produced. In the cases which have fallen under our notice, the disease of the mouth occurred invariably in individuals who had laboured for a considerable time under symptoms referable to gastro-intestinal irritation; the irregular intermittent fever of one class of physicians, and the febris verminosa of another. The local affection commences by an intumescence of the salivary glands, and by an increased secretion of saliva, which becomes changed in its properties, being more fluid, sanious, and of a fetid odour, and during the night especially, runs in a stream from the mouth. The odour exhaled by the saliva resembles very strongly that which results from a profuse mercurial salivation. When the gangrene commences in the gums, these become at first tumid, then assume a livid hue, and separate themselves from the teeth, which readily move in their sockets, and are covered with a grayish mucus. On different parts of the gums, and on the mucous membrane, as it passes from them to the lips, small ash coloured spots make their appearance; these soon become of a deeper colour, and form ulcers, the bottoms of which are of a grayish brown. The ulcers augment rapidly in size, laying bare the aveolar process, and even a portion of the maxillary bone. The teeth fall out, and not unfrequently considerable portions of dead bone are detached. The destruction of the soft parts extends to the lips and cheeks, not however by a process of ulceration—large portions becoming at once affected with gangrene, sometimes dry and at others humid. The parts swell, assume rapidly a grayish or black colour, and separating, deprive the mouth not unfrequently of the greater part of its parieties.

In other cases the disease appears under a different form, commencing in the lips or in their immediate neighbourhood. These parts swell to an enormous extent, particularly at the point where the gangrene first occurs. This portion becomes hard, and no longer yields to the pressure of the finger; the skin assumes a smooth shining appearance; its temperature

is augmented, and it has a slight degree of redness, which disappears gradually at its circumference. In a few days a round livid or grayish spot makes its appearance on the part where the tumefaction is the greatest; it increases in size with great rapidity, and is surrounded by a red border accurately circumscribed. This spot very soon becomes black, and in the space of from three to six days the gangrene, in general, extends itself to the greater part of the face, and destroys by its effects the life of the patient. When by an energetic and appropriate treatment, the progress of the mortification is arrested, the dead portions separate, and the parts affected heal more or less promptly.

Dr Richter divides the gangrene of the mouth, according to the difference of its causes and progress, into three distinct species:

1. The scorbutic (*cancer aquaticus scorbuticus—noma scorbutica—stomacace gangrænosa infantum scorbutica*);

2. The gastric (*cancer aquaticus gastricus—noma gastrica—stomacace gangrænosa infantum gastrica*); and

3. The metastastic (*cancer aquaticus metastaticus, noma metastatica—stomacace gangrænosa infantum metastatica*).

The first, or scorbutic species, is, according to our author, by far the most frequent; to it he conceives should be referred all the cases which have occurred endemically in the orphan asylums, or epidemically in situations bordering upon the sea.

The invasion of the disease is slow, and it is in general preceded by a series of debilitating causes, or of some affection by which the organic energies of the system have been impaired. The precursory symptoms are a general debility, listlessness, indisposition to engage in play, or even to move about; the little patient cries frequently, is discontented, and desires to sleep, without the ability. The countenance is pale and dejected, and a puckering of the cheeks about the corners of the mouth is observed. The child becomes emaciated, and is subject to nocturnal sweats; it has no appetite for food, but in general experiences an increase of thirst. This state of things having continued from eight days to two weeks, the local disease makes its appearance. The patient complains of sharp pains in its mouth and gums; fetor of the breath soon manifests itself, accompanied with an increased secretion

of saliva; a sense of itching or pricking, and heat, is experienced in the gums, which assume a dark red hue, become tumid, and bleed upon the slightest touch. The salivation augments rapidly, accompanied generally with a slight discharge of blood; the salivary glands at the same time become tumid and painful. The appearance of the disease at this stage has a very marked similarity with the affection of the mouth resulting from a mercurial impression, and we have known even experienced physicians to confound it with the latter.

When the local disease is allowed to progress, the periosteum of the gums separate from the roots of the teeth; these become loose, and are covered with a coating of mucus of a dirty white colour. Frequently they fall out of their sockets. It is at this period that fever generally occurs, at first in the latter part of the day, and accompanied with an increase of the night sweats; diarrhœa, also, is not unfrequent. The disease may continue in this state for many weeks, or even months, as Drs Wepfer and Coates have remarked. Most generally, however, in a few days either actual gangrene or gangrenous ulcerations make their appearance. The gums become covered with vesicles of an ash colour, which rapidly enlarge, unite together, and rupturing, show the gums beneath to be black and gangrenous. When ulcerations occur, they present edges slightly inflamed, elevated, uneven, and irregularly circumscribed; their surface is of a gray or rather greenish colour, and rough. When the black or gangrenous portions of the gums separate, an ulcer similar in form to the slough is left, which assumes immediately a gangrenous appearance. The gangrene extends itself very rapidly, laying bare the alveola and inferior maxillary bone. The teeth fall out, and portions of dead bone are thrown off, should the little sufferer not sink under the disease at an earlier period. From the gums the gangrene is propagated to the lips, cheeks, and other soft parts of the face, which become more and more swollen as the disease progresses. This intumescence presents all the appearances of asthenia. At this stage of the disease, and sometimes previously, there is a difficulty in moving the lower jaw, in consequence of which the mouth becomes firmly closed. This Reimann has erroneously considered a

tetanic affection; it, however, evidently arises merely from the swelling of the soft parts, and the pain produced by any effort to move the jaw. Upon the mucous membrane of the mouth one or more grayish spots make their appearance, and in a few hours change into ulcers that present the same characters as those already described, and occasion invariably a great loss of substance. In from three to seven days the mortification will frequently extend itself to the whole of the soft parts surrounding the mouth, which exhale a cadaverous odour, and discharge a putrid sanies. The fever, which now augments in intensity, assumes a nervous or hectic character, and the child dies, generally about the eighth, or at the latest on the fourteenth day after the commencement of the gangrene, its body presenting all the indications of a general colliquation.

The next species of the gangrene of the mouth, or the gastric, is, according to Dr Richter, very rarely met with. He conceives, however, that many of the cases reported by Boot, Lund, A. G. Richter, Cullen, and other authors, were of this kind. It is marked by the following symptoms: The appetite is lost, or it is augmented, and continues so to the death of the patient; the tongue is coated, but apthæ are seldom present. The general system is not affected as in the former species, and the disease attracts but little attention until the gangrene has commenced. The patients preserve until then their accustomed gaiety, play about, and present, in fact, no symptoms from which the approach of so serious a disease as the gangrene of the mouth could be anticipated. The occurrence of this form of the malady is in general sudden; the patient becomes all at once agitated and peevish, and is affected with slight accessions of fever, especially if the intumescence of the face has already commenced. These symptoms are in general accompanied with a sense of uneasiness, nausea, vomiting, diarrhœa, or constipation. The mortification does not commence at the gums, but attacks first one of the cheeks, the commissure of the lips, or the mucous membrane of these parts, from whence it extends, in many instances, when the affection arrives at its height, to the gums and to the bones. The local affection commences in the cheek; this becomes swollen, hard, red, and shining; an increased secretion of saliva takes place, and there is exhaled

from the mouth a peculiar and highly offensive odour. On examining the internal surface of the affected cheek, one or more small ill looking vesicles are perceived, which after some days rupture and form a malignant ulcer of a dirty gray colour. This augments rapidly in size, having a rounded form, with red, distinctly circumscribed edges. This ulceration is not always immediately detected, and occasionally it is entirely overlooked, in consequence of the attention of those about the child being fixed entirely upon the external swelling of the cheek, as well as from the little patient being unable, in consequence of the swelling, to open his mouth to any extent. Even the slight excoriation or fracture of the skin, which often occurs at one of the angles of the mouth, will not lead an individual unaccustomed to the disease to suspect the existence of a formidable gangrene on the inner surface of the cheek, at least not until the former has become changed, as is generally the case, into an ulcer with a dirty gray surface, or into a dark brown pustule, surrounded with a red areola, distinctly circumscribed. When gangrene has commenced in the mucous membrane of the cheek, and the consequent ulceration becomes deeper and more extended, there occurs on the external surface, where the tumefaction is the greatest, a livid spot, surrounded by a red areola; this soon becomes of a darker colour, and increasing in size, extends, finally, in the course of from four to eight days, to the lips, nose, and surrounding parts. Portions of it are soft and of a gray or greenish hue, and present all the characters of humid gangrene, while other parts are completely mumified, hard, and of a deep black. On examining the mouth, the gums are found to be destroyed only in that part opposite to where the gangrene in the cheek had commenced, the teeth there being loose, while the rest of the gums are to all appearance entirely unaffected. The gangrene of the gums in this species of the disease may be considered, therefore, as a consequence of the previous mortification of the cheek, whereas, in the former species, the gums are invariably the part at which the gangrene commences, the jaw being frequently destroyed previously to the cheek being affected. When, however, the gangrene of the mouth has arrived to its highest pitch, it is impossible to discover any difference between the present and preceding species of the disease,

the whole of the mouth and its parieties being equally affected in both. In the progress of the disease, the general functions of the system become more and more affected; the appetite is entirely destroyed; the thirst augments; the symptoms of gastric disorder augment in intensity; the diarrhœa becomes more copious, and the abdomen is distended with gas; the debility of the patient rapidly increases, while the intellectual functions remain unaffected even to the last. The fever assumes finally a nervous character; the little sufferer falls into a soporose condition; symptoms of universal colliquation now manifest themselves, and death occurs.

The form of the disease now described is that treated of by our countryman Dr Jackson under the denomination of gangrænopsis, and it is the one which has occurred the most frequently in our own practice. The phenomena of the disease, as described by Dr Richter, are precisely those which we have observed in the cases that have fallen under our notice, with the exception that in place of the disease occurring suddenly, or without any very manifest precursory symptoms, as remarked by him, it has always been subsequently to some affection of the stomach and bowels, attended with febrile symptoms of an intermittent or remittent type. In no instance, also, have we known the gangrene to attack the lips, or even the whole thickness of the cheek; we were enabled in every case to arrest its progress before it had extended itself beyond the mucous membrane lining the cheek on which it first occurred and a small extent of the gums in the immediate neighbourhood. It is probable, however, that the cases which we have been called upon to treat were, even in their earlier stages, of a much milder character than those to which the author refers.

The third species of the gangrene of the mouth, the metastastic, is, according to Dr R., next to the scorbutic, of the most frequent occurrence. It succeeds ordinarily to the acute diseases of the skin, as small pox, measles, scarlatina, &c. where these have not run regularly through their several stages, or have been suddenly arrested during their evolution. This form of the disease commences in general abruptly, and without being preceded by any particular symptoms from which its occurrence can be anticipated. In every instance, however, the little patient is in a state of more

or less debility—produced by the preceding disease. The gangrene commences in the soft parts surrounding the mouth. There is, in general, first perceived, a hard indolent tumour about the size of an almond, deeply seated, and attended by a slight degree of redness of the skin by which it is covered. Upon examining the mouth nothing unusual is to be discovered. Within two or three days the tumour increases in volume, and the skin externally becomes of a deeper red; the internal surface of the cheek assumes a gangrenous appearance, while an extremely offensive odour is exhaled from the mouth. The local affection is attended with exacerbations of fever towards evening; the appetite of the little patient is seldom much affected. The external circumscribed redness of the cheek becomes in a short time paler, livid, and then of a grayish hue, and surrounded by a red areola, which extends itself as the disease progresses. The gray sphacelated portion becomes frequently in a few hours completely black. If the gums be now examined, it will be found that in the immediate neighbourhood of that part of the cheek at which the disease commenced they are in a state of gangrene; the teeth here soon become loose, and finally fall out of their sockets. This affection of the gums is very often, as in the scorbutic species, secondary to that of the cheek, and does not occur until a considerable portion of the latter and of the lips are in a state of mortification. When the disease attacks the lips, a red spot occurs upon some part of their lining membrane; this becomes by degrees of a deeper colour, then livid, and in a very short period completely gangrenous. Death, the most ordinary termination of this species of the disease, speedily occurs, and hence necrosis of the maxillary bones is rarely observed. As the local affection progresses the general powers of the system sink, a colliquative condition presents itself, and the patient expires without exhibiting any symptoms of a nervous character.

Dr Richter states that within a very recent period he has had occasion to observe a much milder species of the disease, which occurred after an attack of rubeola that had run an irregular course. In this child gangrenous spots, varying in size, of a dark brown colour, and surrounded with a red margin, presented themselves upon the lips near to their angles, as well as upon the cheeks; the general system of the

patient remaining apparently unaffected. These gangrenous patches, which had been preceded for about forty-eight hours by a redness of the parts at which they occurred, appeared to be confined entirely to the skin, for after the sloughs separated there was little loss of substance. A suppuration of a favourable character immediately commenced, followed by granulations and cicatrization. At a single point, however, the right commissure of the lips, the ulceration formed by the separation of the slough was of a greater depth, the whole thickness of the lip being here affected with gangrene: the small opening which resulted was nevertheless quickly closed, the loss of substance being supplied by abundant granulations.

The attempt to establish here any parallel between the peculiar affection under consideration and the destruction of the lips and cheeks by scrofulous, herpetic and syphilitic ulcerations, Dr Richter considers superfluous: the destruction of these parts in the latter cases never resulting, as in the former, from gangrene, but from a genuine process of ulceration, while, also, the existence of the phenomena peculiar to the disease giving rise to these ulcerations are always sufficient to point out their peculiar character.

The gangrene of the internal surface of the cheek, which constitutes, as we have seen, Dr Richter's second species of the gangrene of the mouth, and which has been recently very ably described by M. Guersent, under the denomination of *membranous inflammation of the cheek**, would appear to us to differ in many respects from the gangrenous affection of the mouth commencing in the gums, and hence there is a propriety in describing it as a distinct species. We see no necessity, however, for Dr R.'s third species, the metastastic, which differs from the former merely in the circumstance of the local affection following an eruptive disease. Against the name metastastic, which is founded in what is conceived to be an erroneous pathology, we must be allowed strongly to protest.

As to the exact nature of the local affection of the mouth under consideration, and the cause of its invariable and rapid termination in gangrene, and its general mortality, there still exists much obscurity. Within the short period during which

* See the last number of this Journal, page 215.

the disease has elicited the attention of the profession, a number of opinions and hypotheses, differing widely from each other, have been advanced in relation to these points. The principal of these are examined and rejected by Dr Richter. The opinion, however, which deserves the most attention, principally from the talents and respectability of its advocates, is that which has been advanced recently by Drs Klaatsch and C. G. Hesse. These authors consider that what has been viewed as a gangrenous destruction of the gums and of the soft parts surrounding the mouth, is to be attributed to a decomposition of these parts perfectly analogous to what takes place in the softening of the stomach, uterus and other organs, and which is independent on, and unconnected entirely with, gangrene. This hypothesis is strongly combated by Dr Richter, who maintains that the disease is a simple mortification resulting from a preceding inflammation of the parts in which it is seated, of an asthenic character. He supports his opinion by the following arguments.

1. The fetor exhaled from the mouth during the whole continuance of the disease. This symptom, remarks Dr R., is never observed in cases of softening, properly so termed, of any one of the organs. It cannot exist, because the softening is characterized, not by a chemical decomposition, but in consequence of the tissues of the affected part being reduced to a less complex state of organization. We cannot conceive that the development of the fetid odour can be produced simply by the action of the atmospheric air upon the softened tissue. Without wishing to adopt the opinion which is here contested, we would merely remark that a fetid exhalation, arising from a gangrenous condition of the mucous membrane of the uterus, is very generally, if not always, present in cases of softening occurring in that organ.

2. That the disease is not a simple softening, Dr R. conceives to be proved by its being preceded, for a short period, by an intumescence of the parts affected, of an asthenic inflammatory character, which has been considered by the most skilful practitioners to be the most certain indication of the speedy occurrence of the gangrene of the mouth. Inflammation is not developed after the gangrene has commenced as a *conamen naturæ*—an effort of the parts to resist destruction, as Dr Hesse has supposed; on the contrary, it always precedes

for several days their death. Besides, it yet remains a question to be decided, whether the gelatinous softening of parts may not be the result of inflammation.

3. The organic condition of the parts destroyed by the disease, and the manner in which the destruction commences and spreads, are, according to our author, proofs of its gangrenous character. The phenomena upon which our diagnosis is founded are, he conceives, too characteristic of gangrene to permit us to refer them to any other cause, and especially to simple softening.

4. Dr Richter conceives that the necrosis by which the maxillary bones frequently become affected in this disease, when it has commenced in the gums, or when spreading from the lips or cheeks it attacks the soft parts in immediate contact with the bones, occasioning large portions of them to separate, and the teeth to fall out, can only be explained by supposing the bony structure to participate in the disease of the soft parts, and to become like them destroyed by gangrene.

If the destruction of the soft parts were the effect simply of a gelatinous softening, the bones participating in this condition should likewise present, to a greater or less degree, a similar softening. This argument does not appear to us, however, one of great force. A bone may certainly become necrosed in consequence of disorganization of the soft parts by which it is surrounded, including its periosteum, from whatever cause this disorganization may arise. The death of a bone is not conclusive evidence that the destruction of the soft parts enveloping it has been the result of gangrene.

5. Dr Richter maintains that the disease of the mouth is gangrenous, from the fact, that the sphacelated portions of the gums, cheek and lips are surrounded by a distinct line of demarcation, or a border of a deep red colour, which changes to yellow in those cases in which the dead parts separate, and suppuration of the edges takes place. This, he remarks, never occurs excepting in cases of gangrene; it is observed in no other species of destruction to which the soft parts are liable.

The foregoing arguments are considered by Dr Richter as sufficient to disprove those advanced by Drs Klaatsch and Hesse in favour of the identity between the disease of the mouth under consideration and the gelatinous softening and putrescence

of the uterus, drawn from the circumstance of these two species of disorganization occurring during infancy; the slight affection of the general system by which they are accompanied, compared with the serious character of the local affection, both affecting parts in the neighbourhood of natural orifices, or rather parts which have a very great similarity in their organization, &c. These circumstances Dr R. considers merely as accidental, and from which no evidence can be drawn in proof that the two diseases have the same pathological character.

Our author is of opinion that the malignant nature of the gangrene of the mouth in children is to be explained principally from the peculiarity of the infantile organization. As we have remarked in the commencement of this article, Dr Richter considers the disease to be merely a variety of the disease of the gums known to the older physicians under the name of stomacace, from which it differs only in the early commencement of gangrene and its rapid extension to the surrounding parts. It is rare, he remarks, that the stomacace presents itself in this aggravated and alarming form, even in infants. It has frequently occurred epidemically, affecting at the same time a great number of infants as well as adults, without in these epidemics the gangrene of the mouth presenting itself in a single instance; which he considers a conclusive evidence that the state of the atmosphere is insufficient of itself to give to the disease the peculiar malignancy which forms the distinguishing character of the affection we are considering.

Dr Heuter, however, contends that the gangrene of the mouth is a distinct disease from the stomacace. The latter term has generally been employed to designate a disease of the gums and mucous membrane of the mouth, in which the former are tumid, livid, and covered with superficial ulcerations. Should several of these ulcers unite, and become gangrenous, destroying the soft parts and the bone itself, the disease would then assume a character identical, it is true, in some respects with that of the gangrene of the mouth; but even then the slowness of its progress would indicate that it is still a different disease. Dr Heuter is not aware, however, of a single instance in which the character above indicated has been as-

sumed by the stomacace. I am well aware, he adds, that the gangrene of the mouth commences sometimes on the internal surface of the cheek, the entire thickness of which latter is often destroyed by it, so that occasionally the exact nature of the case is often overlooked until a gangrenous slough is formed on the outside of the cheek. But in these cases the gums are not affected until after the complete destruction of the soft parts, and sometimes remain healthy notwithstanding the gangrenous condition of the surrounding tissues. This, he conceives, proves that gangrene of the mouth is an affection entirely different from that termed stomacace, in which an affection of the gums is a primary and leading phenomenon.

In those cases in which gangrene of the mouth succeeds to intermittent fever, Dr Heuter conceives it rational to suppose that both diseases are the effect of the same morbific cause acting upon the system. The affection of the mouth being probably determined by the predisposition to disease in the gums of children, owing to the irritation of teething. This irritation, he remarks, which occurs with the first dentition, and is again induced by the appearance of the second set of teeth, may predispose to gangrene of the mouth when the system is placed under the influence of any morbific cause; the gangrene having considerable analogy to that of the toes in old people, with this difference, however, that the former takes place at a period of life when, and in parts where, the powers of reproduction are very active, the gangrene being the result of a sur-irritation, whereas the mortification of the toes in old age is the result of a defect of action and of nutrition.

The views of this gentleman in regard to the pathology of gangrene of the mouth appear, however, to be very confused and unsettled, for, in another part of his essay, he contends for the retention of the German appellation of the disease, aqueous cancer; maintaining that this points out its proper character, there being no doubt in his mind that the cancer of the lips in adults and the gangrene of the mouth in children are one and the same affection. For the reasons he adduces for this belief we must refer to the essay itself; to us they appear, however, weak, and altogether inconclusive.

The patients in whom the gangrene of the mouth has been

observed are, according to Dr Richter, invariably young children of a very delicate constitution, of a scrofulous diathesis, with soft and flaccid muscles, pale skin and light hair, and in whom, very generally, the functions of assimilation and nutrition have been depressed from a series of causes more or less prejudicial to health. Under such circumstances, the occurrence of stomacace being induced by the debilitating agents to which the children have been subjected, we need not be surprised at the speedy occurrence of gangrene, and the formation of gangrenous ulcers in the interior of the mouth. The inflammation, of a highly asthenic character, by which the disease is preceded, the early formation of patches of a pale and livid colour, and the rapidity with which the gangrene extends itself, all sufficiently indicate the extreme debility of the parts in which the destruction of life takes place. In adults the malady seldom runs on to the same extent as in children, because in the former the system possesses sufficient energy to resist the destructive influence of the debilitating causes to which it is exposed.

The examinations of the bodies of those patients who have been destroyed by the gangrene of the mouth are too few in number, and have been performed with too little accuracy, to furnish any facts calculated to throw light upon the nature of the disease. So far as our own observations go, which, however, have been very limited, the internal organs, especially the stomach, intestines and liver, have presented indications of inflammation of a more or less chronic character. Dr Anderson, physician to the children's asylum of this city, informs us that in the fatal cases of the disease, where he has been enabled to examine by dissection the bodies of the patients after death, he has detected very considerable enlargement and hardening of the mesenteric glands, a scrofulous condition of the glands of the neck, and in every instance a diseased condition of the lungs. In general, the substance of these organs were thickly studded with tubercles in various stages of inflammation and suppuration.

Is the disease contagious? To this question no satisfactory reply can be given. Under certain circumstances, and during particular seasons, a number of infants, similarly exposed, have been affected with it simultaneously or in rapid

succession; it is evident, therefore, that the disease may occur as an endemic or even as an epidemic; but whether it be communicated in any instance from one individual to another, we have too few well authenticated facts positively to determine. In every case, however, as a prudent precaution, it will be proper to separate as much as possible the individuals affected from those in health, as well as from each other; a number of patients labouring under gangrene of the mouth being confined in the same room cannot fail, even with every precaution in regard to cleanliness and ventilation, to render the disease, to a certain extent, more unmanageable than when the patients are placed each in a separate apartment.

In regard to the predisposing causes of the gangrene of the mouth, these are considered by Dr R., in common with nearly every writer who has treated upon the disease, to be the period of infancy, a lax and delicate constitution, and exposure to the influence of various debilitating agents. By a reference to all the observations on record, it will be found that in no instance whatever has the disease presented itself in children of a robust and healthy constitution. Although in some species of gangrene of the mouth the general health of the little patient is but little affected, and notwithstanding the disease occasionally occurs suddenly, yet, in every instance it will be found that the health and strength of those affected have been previously undermined by the influence of various debilitating causes. We seldom find the gangrene of the mouth to occur excepting among the children of the poorest classes; never in the children of those who enjoy, to a sufficient extent, the comforts of life, and to whose nursing a proper degree of attention has been paid. The causes which are enumerated as those favouring the production of this disease are, insufficient or unwholesome food, the use of immoderate quantities of cheese, of salted aliments, of bacon, fish, &c. The influence of a damp or impure atmosphere, or that rendered impure by a number of persons crowded in small, low, and ill-ventilated apartments. Personal and domestic filth. Want of sufficient exercise, and various chronic diseases. As the children of those who live on the borders of the sea are exposed at one and the same time to many of these causes, the gangrene of the mouth is an affection extremely prevalent

in Holland, Sweden, and many parts of England. The influence of long continued exposure to a damp climate, and the constant use of unwholesome food in the production of this disease, are sufficiently established by the report of the physicians attached to the French army which occupied Spain; they having had frequently to treat a gangrenous inflammation of the mouth which occurred among the soldiers, and which received from the Spaniards the name of *fegar* or *fegarite*. Those physicians, also, who have the charge of the poor in large cities met with many cases of the disease every year. In Berlin, according to Dr R., it is seldom that the annual bills of mortality do not contain several deaths from this affection. In the hospitals destined for the reception of children, gangrene of the mouth frequently occurs as an epidemic; in the children's asylum of Philadelphia, Dr Coates remarks that at one period, among the two hundred and forty inmates of the institution, seventy were affected with the disease. Various fevers, whether verminose, intermittent, or mucous, remarks Dr R., which in consequence of their duration and their malignancy are succeeded by a great prostration of the patient's strength, may not only cause a stomacace to assume the gangrenous character, but may also, considered as an imperfect crisis, be the sole cause by which the gangrene of the mouth is induced. All of these causes are considered to act with the greatest intensity when the children exposed to them are of a lax and delicate constitution, with a pale skin and light coloured hair; such also sink the most readily under it, as their systems possess too little energy to enable them to resist effectually the extension of the gangrenous affection.

Gangrene of the mouth is in fact almost entirely confined to the period of childhood. It occurs most commonly at some period between the first and tenth year, but especially between the second and fourth. Seldom, if ever, has a case of this disease been observed in infants during the period of suckling. Fischer, it is true, cites one case which occurred in an infant of nine months, but he does not inform us whether the little patient was weaned, or still nourished by the milk of its parent. The very rare occurrence of the disease in adults is to be attributed, Dr R. presumes, to the greater degree of vital energy possessed by the constitution, and the

greater firmness of fibre, in consequence of which more resistance is presented to the influence of the debilitating cause by which the disease is produced. We do not possess sufficient data to enable us to determine which of the two sexes is most liable to be affected with it.

Dr Richter places among the most ordinary and fruitful of the remote causes of gangrene of the mouth a scorbutic affection occurring during infancy. In those countries particularly exposed to a humid state of the atmosphere, and where the gangrene of the mouth is most frequently met with, as for instance Poland, Denmark, Norway, Sweden, England, Scotland; as well as in situations along the borders of the sea, and in those which from any cause are exposed to occasional inundations, or to a superabundance of moisture, the scurvy is a disease of ordinary occurrence; it reigns there as it were epidemically, particularly among such of the inhabitants as live principally upon salted provisions, or those of an unwholesome or innutritious quality. Sea fish, the fat and oily part of birds, cheese, bad bread, constitute articles of food, particularly when eaten without a sufficient amount of fresh vegetables of a wholesome kind, peculiarly pernicious during childhood, and when at this age the diet is composed almost exclusively of them, they induce in the system a morbid condition; more especially when at the same time it is exposed to the influence of other deleterious causes, as an impure and humid atmosphere, confinement in small and filthy apartments, an inactive life, &c. Hence an improper diet is to be ranked among the predisposing causes of the disease of which we are treating.

Gangrene of the mouth is no doubt in a majority of cases dependent upon gastric irritation. The causes which we have already enumerated act in its production by disturbing the healthy condition of the stomach and other organs appropriated to digestion and assimilation. The apthous ulcerations of the mouth of children, which have in many respects so close a resemblance with those of a syphilitic origin, are always, according to Dr R., the consequence of a morbid condition of the digestive function. When these ulcers, in infants of a feeble and delicate habit, assume a gangrenous aspect, they become in fact genuine cases of gangrene of the mouth,

notwithstanding the general condition of the system may present no indications from which the near approach of so serious and fatal an affection could be anticipated. As another fruitful source of the disease Dr R. enumerates the previous existence of febrile diseases, especially the exanthematic: an imperfect crisis, or the sudden suppression (metastasis) of the affection of the skin would appear, he remarks, to favour in an especial manner the development of the gangrene of the mouth. So frequently has it been observed to occur after the acute eruptions, that the circumstance cannot with propriety be considered as merely accidental.

Diseases of a catarrhal and rheumatic character, which so commonly precede the appearance of a mild stomacace, may, according to Dr R., be regarded as causes capable of imparting to this disease a more malignant character, and in causing it to pass rapidly into a state of gangrene, particularly if at the same time the patient has been exposed to the influence of debilitating agents.

It is probable that in addition to the above, more extended and accurate observations will discover other causes by which under certain circumstances the gangrene of the mouth may be induced.

We have given above the account presented by Dr Richter, of the nature and causes of this affection; this accords tolerably well with the phenomena of the disease, and the circumstances under which it has occurred within our own notice, and is in conformity with the opinions of the majority of those who have within the few past years presented us with an account of their observations and experience in relation to it. From the older writers little satisfactory can be learned as to its pathology. Bidloo, who formed his opinion from the symptoms of three cases only, attributes it to the inflammation of a diseased lymphatic vessel, while most of the others consider it merely as a species of scurvy. The principal authors of a modern date who differ from Dr R. as to the nature of the disease, are Klaatsch and Hesse, whose views we have already noticed. These views are also adopted by Weigand, in his Dissertation published at Marbourg in 1827, while Lentin, a Swedish physician, has denominated the disease *cheïlocace,* or scrofulous tumour of the lips.

Dr Ch. F. Fischer, who deserves great credit from being

among the first writers who directed in a particular manner the attention of the profession in Europe to this affection, endeavours to point out the relation which exists between it, the erysipelas of new born infants, and the induration of the cellular tissue, occurring at the same period. He reports four cases, two of which occurred subsequently to measles, the third after an attack of fever of an asthenic character, and the fourth after a scorbutic affection. All these cases terminated fatally. He concludes that the disease is caused by an induration of the cellular tissue, resulting from asthenic inflammation, attended by infiltration. Dr Seibert, who has traced the diagnosis of the gangrene of the mouth with much skill, considers it to be a different disease from the affection of the mouth denominated stomacace. He conceives that a scorbutic tendency in the system is absolutely necessary to the production of the gangrene, but that the scorbutic character is only fully developed in consequence of the reaction which takes place in the system when the local affection has reached its acme, and hence it is very frequently taken for the effect of the former rather than its cause.

In treating of the diagnosis of the gangrene of the mouth, Dr Richter remarks, that every physician who has had an opportunity of seeing the disease, agrees that it is one which, if not entirely beyond the control of medical treatment, speedily produces, in at least the majority of instances, the death of the patient. By the fifth, or at furthest, the fourteenth day from its invasion, the patient ordinarily sinks under the state of exhaustion which then suddenly occurs; a circumstance which has excited the surprise of all; the general health of the system being so little impaired that to those unacquainted with the disease such a termination would scarcely have been anticipated. Our own experience does not accord with this statement of our author; we have not found the disease so unmanageable as he, in common with others, has reported. We have very generally, as we have already stated, been enabled to arrest speedily the progress of the gangrene, and of the many patients affected with the disease which have fallen under our care, have lost but few. This is also the experience of Dr Coates, as related in the paper referred to in the commencement of this article, as well as of Dr Anderson, the present physician to the children's asylum in

this city, where frequent cases of the disease occur, and of which few terminate fatally*. We can readily believe that when the real character of the disease is misunderstood; when it is overlooked in its first stages, or treated by inert or improper remedies; its termination will very generally be fatal. It is important, therefore, that the physician should be perfectly aware of the circumstances under which this affection of the mouth ordinarily occurs; that he should, when these are present, make frequent examinations of the mouths of the children placed under his care, and on the slightest appearance of disease prevent its further progress by the remedies which have been found most successful in its management.

It is yet difficult to determine with certainty, observes Dr Richter, why this affection terminates so generally in death. The remark of M. Berthe that there still exists great uncertainty as to the proper mode of managing the disease, deserves particular attention; but this uncertainty cannot be viewed as the only cause of its general mortality. Neither, likewise, is the age of the patient, nor the extent of the mortification, sufficient to explain its fatality. We see patients from the earliest to the latest periods of infancy die from the gangrene of the mouth, while others recover after the destruction of a considerable extent of the soft parts, or even the death of large portions of bone. Saviard states that he lost every patient affected with the disease under seven years of age; the observations of others prove, however, the contrary. The same may be said in regard to the treatment; means of the most

* The following statement will show the mortality which has attended this disease in the institution under the care of this gentleman, for a period of nearly three years. It is copied from the register kept in the house.

From June 1st to January 1st 1828 there occurred thirteen cases of gangrene of the mouth, of these *two* died.

From January 1st 1828 to January 1st 1829, there occurred thirteen cases, of these *three* died. Appended to the annual report for this year is the following statement by the resident student of the house: "One child only died from this affection unconnected with other diseases."

From January 1st 1829 to January 1st 1830, there occurred six cases of gangrene of the mouth and two of gangrene of the ear; this latter is an extremely rare disease, these being the only cases that appear from the register to have occurred in the institution. In the annual report of this year it is stated that no death has occurred from gangrene of the mouth as a primary disease.

diverse character have been employed with success in some instances, whilst in others they have failed. The period, however, when the treatment is commenced has been fully proved to be an all important circumstance to insure its success; this, perhaps, more than any other will determine, in the majority of cases, its greater or less mortality. When the physician has been enabled once to determine the period of development, and the subsequent progress of the local disease, he will very generally be enabled to direct his remedies so as effectually to arrest its course and to save the life of his patient, provided he is called in sufficiently early. This is proved by our own experience and that of Dr Coates.

Muys, Lund, Seibert, Klaatsch and Reimann assert, that they very generally succeeded in curing the gangrene of the mouth when it commenced by a tumour in the cheek, or by a livid spot.

The causes by which the gangrene is produced would appear also to have not a little influence upon its favourable or unfavourable termination. It requires, however, more extended observations to arrive at any positive conclusions as to this point. When gangrene of the mouth occurs subsequently to affections of the stomach, it would appear to Dr Richter to be of a less malignant character than when it succeeds to fever or diseases of the skin in individuals of a scorbutic habit. The disease is also peculiarly destructive to life when it occurs as an endemic. In the hospitals destined to the reception of children, and the asylums of orphans, when the disease makes its appearance, it very generally terminates fatally, at least in Europe.

In children who are constantly exposed to the influence of causes injurious to health, gangrene of the mouth is very seldom recovered from.

Numerous cases are undoubtedly reported by different physicians in which the disease terminated favourably, even though in some the destruction of the soft parts about the mouth has been very extensive, and at the same time portions of the maxillary bones have been lost by necrosis. It is nevertheless true, that whenever the gangrene is of any extent there is little to be hoped for, the recovery of the patient being a rare occurrence; the restoration of the parts destroyed always de-

manding a degree of vital energy which does not always exist in the delicate and enfeebled system of the little sufferer.

The chapter in the treatise of Dr R. which is devoted to a consideration of the treatment of gangrene of the mouth is the least satisfactory portion of the work. It contains chiefly an enumeration of the various and discordant remedies which have been proposed by different physicians. We are presented with no satisfactory comparison of their effects, nor of their relative efficacy.

The administration of internal remedies would appear to have in general interfered but little with the progress of the disease, and in no degree have they tended to arrest its fatal termination. In fact the remedies which have been usually employed we might expect, *à priori*, from their nature and the period of the disease at which they were had recourse to, to be little calculated to remove the condition of the system upon which the occurrence of the local gangrene depends, or in any degree to prevent its rapid spread. Those which have been the most warmly recommended are the antiscorbutics, various tonics and purgatives. Boot, the first English writer who has given any account of the disease, directs a drink composed of sarsaparilla and other vegetable substances, in great repute during the period in which he wrote, as purifiers of the blood. Van Lil recommends barley water acidulated with lemon juice. Bruinemann, Bernstein and Wendt, the sulphuric acid diluted with honey. Meza attributes a decided efficacy to the spirit of cochlearia, combined with honey, &c. The chief remedy, however, which has been depended on is cinchona, either alone or combined with other tonics, or with the sulphuric or muriatic acid—while a few authors declare that they have succeeded in curing the disease by purgatives and emetics. "The antiscorbutics," remarks Dr Richter, "possess in general properties of too little activity to render them of any advantage in gangrene of the mouth. The sulphuric acid alone, or in combination with the bark, produces in general no effect, unless received into a healthy stomach and properly digested. In the scorbutic variety of the disease the muriatic acid is too debilitating." The phosphoric acid, the juice of the lemon with beer, Rhenish wine and sugar, are, he conceives, remedies better adapted to the case, and agree better with the stomach; they are to be

viewed, however, only as accessory means, having little influence upon the progress of the gangrene. But when by appropriate local applications the extension of the disease has been arrested, in every form of the complaint the administration of those preparations of the bark the most readily digested, as the infusion, the extract prepared without heat, the sulphate of quinia, &c. becomes indispensable in order to support the strength of the patient, and to enable his system to sustain the efforts necessary to the restoration of the parts which have been destroyed by the disease. At the same time a light and nutritious diet should be directed. So far we believe the directions of the author to be highly judicious; but when he remarks that in every species of gangrene of the mouth the employment of vomits and purgatives will *frequently* be of advantage, we must beg leave entirely to differ from him. If instances do occur in which these remedies are appropriate, they must be extremely rare; we have never met with them: on the contrary, the cases of the disease which have fallen under our notice were all accompanied by well marked symptoms of irritation of the stomach and bowels, and after the gangrene had occurred, with so much debility in the general system, that emetics or purgatives could not fail to have been in the highest degree injurious.

The remedies which have been employed externally with the view of arresting the disease are very numerous. All of them are reported to have been successful in the hands of some physicians, while in those of others they have entirely failed. An enumeration of the principal of them may not be useless or uninteresting.

Unguentum Egypticum.—This ointment, made by boiling together verdigris, honey and vinegar, was a great favourite with Boot and others, as a local application in gangrene of the mouth. The former employed it mixed with equal portions of the syrup of mulberries, the diseased portions of the mouth being first washed with a solution of alum in wine. Boot informs us that by the use of these remedies he obtained a cure in three cases, notwithstanding the gangrene in one of them had made so frightful a progress that it destroyed a portion of the jaw and ten teeth. Van de Voorde directs the same ointment in combination with a vinous decoction of scordium,

absinthium, scabiosa, agrimony, the flowers of the lesser centaury and St John's wort, to which were also added spirits of wine and the muriate of soda. Muys employed likewise the Egyptian ointment in combination with gum lac, ammoniac, the spirit of cochlearia, and a slight addition of alcohol.

Gargles.—A gargle composed of the roots of the aristolochia and the Florentine iris, with agrimony, hoarhound and other herbs, infused in white wine, with the addition of myrrh and aloes, is extolled by Van de Voorde; while Boot directs a gargle made from a decoction of chervil, cinquefoil, myrrh, rose leaves, sage and white vitriol. Savaird was accustomed to wash the mouth with spirits of wine, in which a certain quantity of camphor, alum and sugar was dissolved; or with a decoction in wine of pongranate peel, rose leaves and sumach. During the existence of inflammation Van Swieten directs the mouth to be washed with a solution of the muriate of ammonia and nitrate of potash, with the addition of vinegar or lemon juice, and during a later period of the disease with the spirit of cochlearia and theriac. Van Lil recommends as a gargle a mixture composed of a solution of the muriate of ammonia, gum lac, myrrh, spirit of cochlearia and honey of roses, and subsequently of a decoction of wild roses, with the tincture of myrrh and honey of roses. By recent physicians these gargles are seldom employed; they have little effect upon the progress of the disease, and their proper use is always attended with difficulty.

Astringent washes.—Symmonds employed as a wash to the diseased parts a mixture of port wine and tincture of myrrh. F. Wendt, an infusion of aromatics and arnica, with vinegar and camphorated spirits. Rey, the tincture and decoction of Peruvian bark, with the solution of camphor in alcohol. Pearson, calcined alum, with a decoction of bark and the sulphate of zinc, tincture of myrrh, lime water and alcohol. Under the head of astringent washes, Dr Richter notices the use of a strong solution of the sulphate of copper as recommended by Dr Coates; in his general summing up, however, of the local treatment best adapted to the disease, as we shall presently see, he does not include this wash among his list of remedies. It is one, nevertheless, which presses itself strongly upon the attention of the profession. In the great majority of cases in

which we have employed it, it has succeeded in very speedily arresting the extension and progress of the mortification; while under its use, the gangrenous ulcerations, whether seated in the gums, lips, or cheek, have assumed rapidly a more healthy appearance; and this in many instances in which the disease has been allowed to run on until it has assumed a highly aggravated form.

Sulphuric acid.—Van de Voorde appears to have been the first by whom this remedy was employed in the disease before us. He considered it as more efficient than all the local applications previously in use. He applied it mixed with honey, and asserts that he has found it successful in the most desperate cases. Since his period, the effects of the sulphuric acid have been highly extolled by numerous writers. Van de Weil employed it successfully in conjunction with the Egyptian ointment and the honey of roses. Jourdain combined this acid, diluted, with camphor, muriate of ammonia, honey of roses and alcohol. He relates two cases in which this mixture was successful. Instances are also related by Bruinemann and Courcelles, in which the application of the acid alone produced a cure.

Muriatic acid.—The local application of this acid is stated by many writers to have been found efficacious in the cure of gangrene of the mouth. Van Swieten, upon the recommendation of Poupart, employed it by adding twenty drops of the acid to half an ounce of honey; with a pencil dipped in this mixture he touched the parts affected with the disease. When the case was one of a more aggravated character, the acid alone was used. Invariably he found the extension of the gangrene to be arrested, and the dead portions to separate in a short time. Stelwagen succeeded in curing one case, although a secondary attack, by a mixture of the muriatic acid, extract of cicuta, and the tinctures of myrrh and contrayerva, while in two other cases no advantage was derived from its employment. Siebert, from the good effects he obtained from this acid in five cases, considers it as the most certain remedy for the disease. Bernstein, Richter, Jadelot, Boyer, Isnard-Cévole, Baron and others, recommend it likewise; while Klaatsch and Weigand declare, that from its use they have obtained not the least advantage.

Acetic acid.—Very recently a case has been related by Klaatsch in which the gangrenous portions of the mouth were wet every ten minutes during one night with this acid. In the morning it was found that the extension of the disease was arrested, at the end of the third hour subsequently the sloughs had separated, and the ulcers presented very speedily granulations of a healthy appearance. In two other cases the application of the acid was equally beneficial, but the little patient died in consequence of the occurrence of trismus. In another case in which it was employed by Dr Reimann, the disease was cured. In four days the dead parts separated, and the ulcers went on rapidly to cicatrization. Weigand, however, made trial of the remedy without success. These two instances of failure should not, remarks Dr Richter, prevent a fair trial being given to the acetic acid in gangrene of the mouth, for we well know, few remedies will be found equally beneficial in all cases of any disease. A particular importance should, he conceives, be attached to this acid in the affection under consideration, its empyreumatic nature increasing its antiseptic properties.

Muriate of soda.—This was first recommended as a remedy in gangrene of the mouth by Rey. Two cases are reported in which it was successful. The first was an aggravated case, in which the tincture and decoction of the bark with spirits of camphor had been employed without effect; a pledget of lint, wet with a solution of common salt, was applied to the mouth; the gangrene was arrested; by the sixth day the dead portions separated, and the ulcers were healed in a short period. In the second case the remedy was equally successful. A history of it will be found in the Annales de la Médicine Physiolog. t. iii. p. 307.

Peruvian balsam.—A case is related by E. Thompson, in the London Medical and Physical Journal, vol. lvii. p. 533, in which a cure was obtained by the local application of this article in conjunction with the internal use of tonics and a generous diet of animal food.

Scarification of the gums.—Boot directs the gums to be scarified to give discharge to the putrid blood. Bidloo introduced into the scarifications a solution of the sulphate of copper, and Saviard sulphuric acid diluted with water. Berthe

has supposed this operation so important to the cure of gangrene of the mouth, that he conceives one of the causes of the mortality of the disease is the great difficulty there exists in properly performing it in children.

Excision of the gangrenous portions.—Van de Vorde directs merely the dead portions to be removed, but Jourdain, Berthe, Acrel and Stelwagen have advised incisions to be made into the surrounding sound parts, so as to remove the whole of the disease when seated in the lips. The latter gentleman proposes then to unite the edges of the wound by means of the hare lip suture, and to wet the dressings which are applied with a decoction of scordium, abrotanum, absinthium and vinegar. By this means he succeeded in one instance in obtaining a cure in a few days, and with little deformity to the patient. Boyer, also, advises the excision of the parts affected with gangrene, and, to arrest the hemorrhage, directs the wound to be covered with a sponge wet with eau de rubel and alum; in an hour or two the sponge to be removed, and the mouth carefully washed with diluted muriatic acid. Few, we suspect, will be inclined to imitate this practice.

The actual cautery.—This was employed by Capdeville, Chopart, and Desault; Lund, however, strongly opposes its use in consequence of its not correcting the corrupted state of the fluids upon which he supposes the disease to depend; while Isnard-Cévole considers the actual cautery to be the only means capable of arresting the progress of the gangrene after it has made its appearance. For, he remarks, by increasing the activity of the capillary tissue, it causes a species of local fever which favours the separation of the dead portions of the lips. Baron states, also, that he has cured the disease by this means.

Lunar caustic.—This remedy, which is spoken of in the highest terms by Guersent, in that form of the disease in which the gangrenous destruction commences on the internal surface of the cheek, has been employed by Dr Anderson, of this city, in all cases of gangrene of the mouth which have occurred in the children's asylum since the first of June 1827. Whenever the local affection is preceded by general febrile symptoms, these are treated by bleeding, purging, low diet, and the free use of diluents; as soon as the disease of the mouth presents itself, the nitrate of silver, either in pencil or

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in solution, is applied to the parts affected. When a local inflammation occurs, free and repeated leeching, the application of blisters, poultices, and in some cases, the unguentum sabinæ, this gentleman has found highly beneficial. Until very lately, after the application of the caustic, the parts affected were directed to be wet with the blue wash, as employed by Dr Coates. When so used, it was found, however, to produce a swelling and inflammation of the lips, and of late the caustic alone is applied.

The success of this practice may be judged of by the statement already given of the number of cases which have occurred in the institution, with their results, during the two years and seven months immediately preceding the 1st of January 1830. It is proper to say that the lunar caustic was employed by Dr Coates, but as stated in the paper to which we have referred in the commencement of this review, without any decided advantage. For ourselves, we have never employed the remedy, having always been perfectly satisfied with the effects of the solution of the sulphate of copper; the success, however, which has attended its application in the cases which have occurred of late years in the asylum, presses it strongly upon the attention of the physician.

Having examined all the local remedies which have been proposed for the cure of gangrene of the mouth, Dr Richter concludes, that the local treatment of the disease is that to which most importance is to be attributed. As a local application, he considers that a decided preference should be given to the sulphuric, muriatic and acetic acids, and the muriate of soda. Although he has not been able to convince himself as yet of their good effects, yet from the testimony which has been adduced in their favour, he conceives we have reason to believe that more good may be effected from their use than from that of any other agent. New and more extended facts will be necessary before a decision can be made as to which among them deserves the preference. Dr Heuter likewise believes that the local remedies most to be depended upon are the acids, especially the acetic and muriatic, used in conjunction; while Guersent considers a mixture of muriatic acid and honey to be the very best local remedy in the gangrenous affection of the cheek. It is proper, however, to remark, that the acetic, muriatic, nitric, and sulphuric acids, were fairly tried, as local

applications, by Dr Coates, in cases of gangrene of the mouth; in his hands their effects were by no means, however, so beneficial as they are stated to have been in those of various European physicians.

When the acids are employed, Dr Richter observes, that in every case, in order to obtain from them beneficial effects, their application must be repeated every half hour, or at furthest every hour. They should be applied either by a pencil, or by covering the affected parts with lint moistened with them. Their use should not be discontinued until the gangrene ceases to spread and granulations are formed.

Dr Richter states that he is not prepared to recommend the excision of the diseased portions of the lip, notwithstanding the favourable testimony borne as to the success of the operation, inasmuch as no convincing evidence has been offered to prove the strictly local character of the disease. The actual cautery, however, he considers to be a remedy from which much good may be anticipated, especially in those instances in which the acids fail in arresting the progress of the gangrene, or when the latter invades the whole of the lips and surrounding soft parts. In such aggravated cases the application of the actual cautery should not be confined to the parts already dead, but it should be extended to the live parts with which these are in contact, in order that by exciting in them an increased vital action, the spread of the gangrene may be arrested, and the separation of the sloughs facilitated.

Art. XII.—SOUTHWOOD SMITH ON FEVER.

A Treatise on Fever. By Southwood Smith, M.D., *Physician to the London Fever Hospital. London printed*, 1829. *Philadelphia reprinted. Carey and Lea*, 1830: *pp.* 448.

This work bears internal evidence of being the production of a very able man; and as such we shall feel ourselves justified in giving a full analysis of it.

The treatise is divided into chapters, in which the principal points connected with the philosophy of fever are considered; such as the ancient and modern doctrines relative to the nature and seat of fever, its varieties, common phenomena, and the true principle of arrangement applicable to it; its pathology, whether of the solids or fluids; the theory of its nature, deducible from the phenomena; its causes, whether exciting or remote; and lastly, its treatment.

CHAPTER I. *Necessity of a further investigation of Fever. Ancient and modern Doctrines relating to it. Questions to be solved before Fever can be understood. Precise object of investigation, and proper mode of conducting it.*

Dr Smith, on taking charge of the London fever hospital, had his attention particularly directed by the founders of the charity to two points, conceived to be of chief importance; 1st, the accumulation of facts, by which the true nature of fever might be ascertained; and 2ndly, the cautious trial of remedies by which a more sure and successful mode of treatment for the disease might be discovered. Of the zeal and success with which he has performed his arduous office, the reader will have abundant evidence in the account which we are about to give of his labours.

Every candid physician must agree with Dr Smith in admitting the great obscurity which surrounds the subject of fever. The greatest physicians that have ever lived have admitted the difficulty of the investigation; and if they had directed their genius to the establishment of a few facts on a sure basis, on which their successors might have cautiously built, instead of their attempting to erect the entire edifice of fever, a vain attempt for a single individual, we might not now have cause to regret the extreme vagueness and deficiency of our knowledge on this subject.

In the London fever hospital the greatest pains are taken to accumulate accurate facts relative to the cases admitted. Between six and seven hundred patients annually pass through its wards. The symptoms of each case are carefully noted at stated times, and also in the interval, if changes occur; and any case which terminates fatally, is examined after death, and the morbid appearances recorded. It is on observations made in the course of four years, in the manner and on the

number of patients above mentioned, that Dr Smith's work is founded.

The author begins by giving a rapid sketch of the doctrine of fever held by Hippocrates and adopted with amplification by Galen and Sydenham, that it is caused by morbific matter, to expel which the fever is to be considered as the effort of nature. "That conjectures so gratuitous," says Dr Smith, "and so utterly incompatible with the structure and functions of the animal frame, should, at such distant periods of the world, under such different conditions of society, and in such different states of science, so entirely possess and satisfy the minds of three of the most extraordinary men that ever illustrated or extended any department of science, will appear the less wonderful when we consider that the doctrines relative to fever, which displaced and succeeded them, originated in precisely the same error, and vary in their aspect only in conformity to the progressive advancement of general science."

Dr Smith next notices the theory of Cullen, which being more explicit in its language, and intelligible in its nature, caused the then prevalent doctrines of the humoral pathology to be exploded; though it still embraced an important feature of the humoral doctrine, namely, that fever was an effort, if not to expel a morbid humour, at least to correct a morbid action. The author gives the following masterly sketch of this theory:

"Cullen, building upon the foundation laid by Hoffmann, rivalling in the number of his pupils, and exceeding in the brilliancy of his success, if not in the perpetuity of his fame, any name of antiquity, achieved this great revolution; and in opposition to the ancient theories, taught that the first change induced in the animal system, by the operation of the exciting causes of fever, is a diminution of the energy of the brain; that all the powers of the body and all the faculties of the mind, that the functions of sensation and motion, the processes of respiration, circulation, and secretion, all fail or are diminished in the general debility; that after a certain time, a morbid increase of some of these functions, especially of the circulation, takes place, with an augmentation of the heat; that these three states, that of debility, of cold, and of heat, bear to each other the relation of cause and effect; that the first state is the result of the sedative or debilitating influence of contagion, marsh

miasmata, cold, or any other exciting cause, and the subsequent states are the result of the first; that the debility produces all the phenomena of the cold stage, and especially a spasmodic constriction of the extreme arterial vessels; that this spasm or atony of the extreme vessels exists not only on the first attack of the cold stage, but remains during the whole subsequent course of fever; that the spasm of the extreme vessels throws a load of blood on the central parts of the circulating system, which proves a source of irritation to the heart and arteries, and excites them to a greater action; that this increased action, the source of the heat and the other phenomena which constitute the second or hot stage, continues till the spasm is relaxed or overcome; and that this excitement of spasm for the purpose of producing the subsequent reaction is a part of the operation of the *vis medicatrix naturæ*, the innate preserving power of the constitution." P. 26.

Dr Smith considers the theory of fever of Brown to be essentially the same with that of Cullen, both attributing all fever to debility. But, perhaps, there was this difference between their theories, that while Cullen considered the hot stage not perhaps as debility, but the means of nature for overcoming the debility in which spasm consists, Brown considered debility to characterize every stage of the disease.

The author next notices the more recent views of Dr Stoker, Dr Burne, and Dr Clanny. Dr Stoker's opinion in relation to common epidemic fever, that it is never essentially inflammatory, is deemed by Dr Smith to be precisely the Brunonian doctrine of fever, so far as this point is concerned; but Dr Stoker revives the humoral pathology when he asserts that typhus fever is connected with changes which *primarily* take place in the fluids; and Dr Burne expresses the same opinion more broadly, when he states "that the adynamic fever has no local seat; that its nature is *a morbid condition of the blood*, produced by the operation of the primary cause, the respiration of a contaminated or poisoned atmosphere: that this morbid blood, acting on the brain and nervous system, is of itself sufficient in very many instances to bring about the very great derangement and imperfect performance of all the functions of the organic and of the animal life; while great derangement and imperfect performance of all the functions constitute the phenomena of adynamic fever."

Dr Clanny's views are somewhat different. Instead of re-

garding, with Drs Stoker and Burne, a vitiated state of the blood to be the essence of typhoid or adynamic fever, he considers its proximate cause to be a want of power in the system to form blood. Thus it is perceived that in these doctrines of the nature of a large division of fever, an approach is made, in a modified form, to the old doctrines of a humoral pathology.

Dr Smith next notices a widely different and indeed diametrically opposite theory of fever. According to the views of a large and increasing sect, fever is strictly a local disease, having its primary and essential seat in one organ, and consisting in inflammation of that organ. Dr Clutterbuck is then noticed as one of the most distinguished advocates of this opinion, and the following quotation made by Dr Smith from his work is so well expressed that we think we shall be gratifying our readers by giving it in full.

"Fever, in regard to its effects on the system," says Dr Clutterbuck, "is the most general of all diseases, and gives rise during its progress to the greatest variety of symptoms. These, contemplated in the mass, present nothing but confusion. Like all complicated phenomena, they require to be subjected to strict analysis; that their order may be traced, and their relation to each other and to the existing cause shown. To the neglect of this may be ascribed the error, as I conceive it to be, which has been so generally fallen into, of considering fever as a universal disease, or one that affects for the first time the whole system; no one part being supposed to suffer necessarily before the rest. Whereas, when the disease is minutely scrutinized, and its first appearance accurately noticed (which indeed from the slightness and consequent neglect of the first symptoms is rarely done), it will be found to be strictly a *topical* affection, the general disorder of the system being merely *secondary*, or symptomatic of this."

The author next speaks of the similar doctrine of Broussais, and from the order in which their views are presented, we may presume that Dr Smith thinks that Dr Clutterbuck has the priority over Dr Broussais in the distinct enunciation of the pathological principle, that all fevers arise from a local irritation or inflammation. But while there is a perfect accordance of doctrine as to the local origin of this class of diseases, there is a wide discrepancy of opinion as to the particu-

lar organ affected. Thus, Dr Clutterbuck thinks that the organ universally affected in every variety of idiopathic fever is the brain. Out of fifty cases, the symptoms of which he noted down with the greatest minuteness, no two were found to correspond in the minute points, though they all agreed in a manifest affection of the brain and its functions. As the term *phrenitis* implies delirium, which does not always occur in fever, Dr Clutterbuck proposes *encephalitis* as a proper denomination for the entire class of idiopathic fevers.

On the other hand, Broussais contends that the primary and essential seat of inflammation in fever is the mucous membrane of the stomach, or of the intestines, or both, but especially the former; and that, therefore, the proper designation of it is *gastro-enteritis*. This fact was long admitted with regard to certain fevers called symptomatic, but Broussais extends the assertion to comprehend all fevers, and this is the main feature of his celebrated doctrine, and that feature of it which he maintains to be peculiarly and exclusively his own.

Thus it would appear that the prevailing doctrines relative to the nature and seat of fever, as Dr Smith very justly remarks, are two, the direct reverse of each other; one, that it is a general disease affecting the entire system; that this affection of the system consists in debility, which is manifested first by a loss of energy of the brain, but which extends to every organ and every function; and that consequently *the absence of any primary local disease* ought still to form an essential part of the definition: the other, that it is, in the strictest sense, a *local disease;* that its primary seat is invariably fixed in some one organ; that the affection itself consists in inflammation, and that that inflammation is seated according to one opinion (Clutterbuck's), in the *brain;* according to the other (Broussais), in the *stomach.*

The author next presents some very interesting remarks on the necessarily opposite treatment which these two prominent doctrines lead to. He who believes that debility constitutes the essence of fever, must abstain from every kind and degree of depletion; while he who considers it of an inflammatory nature, must feel himself impelled to resort to the lancet and leeches. Dr Stoker, an advocate for debility, enumerates the remedies for fever in the following order, according to their

relative importance: namely, in *mixed fever*, 1. Cleanliness; 2. Ventilation; 3. Cool regimen; 4. Plentiful dilution; 5. Purgatives; 6. Topical bleeding; 7. Antimonial powders. In *typhoid fever*, 1. Yeast; 2. Wine; 3. Aperients; 4. Emetics; 5. Blisters; 6. Tepid or cold affusion. In this enumeration, it is perceived, general bleeding is omitted, and local bleeding is placed sixth in rank as to importance in mixed fever, and is not enumerated as a remedy for typhoid fever. On the other hand, Dr Clutterbuck, an advocate for inflammation in fever, says, using his pregnant words quoted by Dr Smith, "Fever, to be treated successfully, must be treated on the general principles of inflammation; but, at the same time, with the modifications arising out of the peculiar nature of the organ affected; and, in some degree also, the nature of the exciting cause. Bloodletting, which but a few years ago was looked upon with abhorrence in the cure of contagious fever, and the utility of which is still far from being generally appreciated, is proved by ample testimony to be not only the most powerful, but the safest of remedies." And Dr Broussais teaches, that in every variety and stage of fever, leeches are to be applied to the stomach, and that scarcely any thing else is to be done except enjoining rigid starvation. Emetics, purgatives, bark, wine, are all denounced; and a costive state of the bowels continuing during five or even ten days, so far from requiring interference, is, according to him, a good symptom.

Dr Smith seems to consider these various doctrines to be carried to extremes, and, therefore, all to include a certain portion of error. Thus he says,

"All the partial and imperfect views of fever which have now been brought before the eye of the reader, originate in one or other of the following errors, obvious as they all are: either that of assuming as a fact what is merely a conjecture; or that of assigning to the genus what belongs only to the species; or that of characterizing the disease by what appertains only to a stage; or that of mistaking the effect for the cause. On careful examination, it will be found that one or other of these errors, which are as serious as they are palpable, has vitiated, in a greater or less degree, every generalization of fever that has hitherto been attempted." P. 42.

Thus, continues Dr Smith, the believers in debility derive their notion of the whole disease from the phenomena which

occur in the first and last stages only; overlooking the intermediate stage, in which there is generally the most unequivocal indications of increased sensibility in the nervous, and increased action in the vascular system. Again, when they contend that debility is the essence of all fevers, they affirm what is indisputable of fevers in particular seasons, climates, and constitutions; but beyond this their generalization fails.

Again, the advocates of the opinion that the essence of fever consists in a morbid condition of the blood, assume something as a fact which has not been yet proved. The blood may be diseased in fevers, but Dr Smith contends that those who assert this fact do not *know* it to be so, have not appealed to chemistry in such a way as to arrive at any certain knowledge. But even admitting that changes take place in the blood, they occur too late in the disease, according to the author, to be considered as constituting its essence.

Thus, also, in attributing fever to inflammation of the brain as the sole cause, the error has been fallen into of assigning as a character of the whole genus what only belongs to a species; and a similar error is committed when inflammation of the mucous membrane of the stomach and intestines is assigned as the sole cause of fevers; for while such inflammation is sometimes absent, it is, when present, in innumerable instances, an effect and not a cause.

In this manner Dr Smith undertakes to point out in what respect the more celebrated theories of fever are defective, and proceeds to remark that the subject cannot be understood until we obtain exact answers to the following inquiries: 1. What is the series of phenomena which constitutes fever? 2. What are the particular phenomena which are common to all its varieties? 3. What is the order in which these phenonomena occur? 4. Which are the organs, and what their states, upon which these phenomena depend? 5. What are the external signs of these internal states, or what the indications by which their existence may be known? 6. What is the external noxious agent or agents, or the exciting cause or causes of the disease? 7. What is the particular remedy, or the particular combination of remedies, which is best adapted to each state of each organ? Dr Smith does not flatter himself that he is able to answer all these questions, although he

thinks that sufficient information is possessed to answer several of them with a high degree of probability.

Conceding that the phenomena of fever, that is, the events which are obvious to our senses, have been minutely observed and faithfully reported, and that, therefore, the first inquiry in the above list is answered; still, as many of these events are observed to be often absent, while it cannot be doubted that fever is nevertheless present, Dr Smith conceives that we must necessarily proceed to the second inquiry; namely, what is that particular combination of events which is essential to the situation of the disease? In singling out this particular series of events we are liable to several sources of error. Thus we may enumerate too few; or mistake the adventitious for the essential; or place them out of their true order, supposing that which is antecedent to be sequent, and, consequently, assigning that as a cause which is really an effect.

Dr Smith proceeds:

"The first thing to be done, then, is to ascertain the concourse of symptoms; and the second, to determine the order in which they occur. When these two points have been made out, what is essential and what adventitious, as well as what is the cause and what the effect, become at once clear and certain. But the difficulty lies in discovering, amidst the infinite diversity and contrariety of symptoms, when we may safely assure ourselves that we are in possession of all the essential phenomena. Our guide is *invariableness* of concurrence. If we can ascertain that a certain number of events *invariably* take place in every form and degree of fever, these events will give us the particular phenomena which are common to all the varieties of the disease. If we can further ascertain that these events invariably concur in a certain order, we shall have discovered what events bear to each other the relation of cause and effect. And the establishment of this relation of events, their constant connexion with each other, the uniform antecedence and sequence appears to me to be the only theory after which it is consistent with the principles of sound philosophy to search. If I have endeavoured to establish this connexion, and have thus ventured, as I conceive, in a strictly philosophical sense, to propose a theory, in doing so I have carefully restricted myself to the attempt to deduce a legitimate conclusion from facts previously ascertained. It does appear to me that these three points, namely, the common phenomena, the invariableness of their occurrence, and their mutual

relation, are satisfactorily established. Whether I shall be able to communicate this conviction to the reader I do not know; but I hope that he will at least coincide with me in opinion, that this mode of investigating the disease affords us the best chance of arriving at satisfactory results." P. 48.

In this extract our readers are presented with a view of Dr Smith's system of philosophizing in relation to fever. How far he has made it available in producing a masterly analysis of his subject, abundant evidence will be furnished in the progress of our review.

CHAPTER II. *Varieties of Fever. Common Phenomena. Organs always diseased, and Functions deranged in Fever. Fever not Inflammation. Common Phenomena of Fever exemplified in Plague, Yellow Fever, &c. Received Classification and Nomenclature defective. True Principle of Arrangement.*

The importance of the author's second chapter is shown by the above synopsis of its contents. It opens by calling attention to the numerous varieties of fever, and to there being, nevertheless, a *something* which establishes the identity of the disease under every diversity of aspect, whether it be the yellow fever of the West Indies or the plague of Constantinople or of Egypt. This *something* must embrace the common phenomena, and what these are the author proceeds to inquire.

Boerhaave undertook to ascertain the common phenomena of fever by collecting the symptoms observed in a great variety of fevers, and then excluding those not common to all. Proceeding in this way, he was surprised to find his catalogue ultimately reduced to three, namely, shivering, frequent pulse, and heat. To this catalogue Cullen makes the following additions:—"languor, lassitude, and other signs of debility, together with derangement of the functions, particularly a want of vigour in the limbs, *without any primary local affection.*"

Dr Smith very justly remarks that Boerhaave's common phenomena are grossly inaccurate, as shivering, frequent pulse, and heat, are each absent in certain fevers; and he thinks that that part of Cullen's definition given above in italics has so direct a tendency to mislead the mind, and positively to prevent it from observing the real phenomena of the disease, that he questions whether the introduction of this single phrase

into the definition of fever has not been the occasion of much practical mischief. For our parts we heartily concur with Dr Smith in this statement; for whether we consider ourselves Broussaists or Anti-Broussaists, or disclaim to be the disciples of any particular school, we must concede that nothing could be more unfriendly to the progress of pathology than views of disease emanating from high names that should discourage attention to the condition of the internal organs.

"This total failure of men" of unquestionable acuteness and splendid genius "in their attempts to discover the common phenomena of fever, affords a strong presumption that they have not pursued this object in the right path. Without doubt before it is possible to succeed in any scientific investigation, it is necessary to form a distinct conception of the object of the inquiry. Fever is not an entity, not a being possessing a peculiar nature; and the object of investigating it is not to discover in what such nature consists, or what it is that constitutes its essence; but fever is a series of events, and the object of inquiry is to discover what the events are; what the events are that invariably concur in the series; and in what order they constantly succeed each other. When we have discovered this we have ascertained all that we can ever know of what is termed the nature of fever, as it is this, and only this, that we can ever know of any object or process." * * * * "Every natural process consists of a number of events, and our knowledge of that process is complete when we have ascertained the events themselves, the order of their succession, and the events to which they give occasion. We can make no real progress in knowledge unless we keep steadily in view the kind of information which it is possible to acquire, and which it is to our purpose to seek, and dispossess our minds of the phantoms which have so long enthralled and abused them." P. 58.

The author next enters upon the inquiry, what are the events which invariably concur in fever? And here we are very properly cautioned not to suppose that symptoms are events. Symptoms are only indications of events, and are the external and visible signs of internal, and, for the most part, as long as life continues, invisible conditions of the organs. It is, therefore, to the state of the organs that we must look for the events which constitute fever.

"Are there any states of any organs that always exist in fever? Are the states constant? Are the organs affected constant; and can both be ascertained? If this can be truly an-

swered in the affirmative; if it can be proved that there are certain conditions of certain organs which invariably exist in fever, in every type, in every degree, in every stage of it, we shall have arrived at a satisfactory conclusion relative to the first part of our inquiry.

"The evidence is as complete as observation during life and inspection after death can make it, that a morbid change does take place in a certain number of organs in every case of fever, from the most trivial intermittent to the most alarming continued fever; from the mildest plague to the most malignant typhus; that at the two extremes of this scale, and at all the intermediate gradations of it, there are certain organs which are always affected, and that the affection in all is similar.

"The identity of the organs is inferred from the indications they give of disordered function during life: the identity of the affection is inferred from the similarity of morbid appearances which they exhibit on examination after death.

"The organs affected are those which constitute the nervous system; those which constitute the circulating system, and those which constitute the systems of secretion and excretion. The spinal cord and the brain; the heart and the arteries, especially their capillary extremities; the secreting and the excreting organs, which in fact are composed, essentially, of the capillary extremities of the arteries; the secreting and the excreting extremities of these arteries, especially as they terminate in the external skin, and in the mucous membranes which form the internal skin,—this is the chain of diseased organs: derangement in the nervous and sensorial functions; derangement in the circulating function; derangement in the secretory and excretory functions,—this is the circle of morbid actions.

"There never was a case of fever in which all these organs and affections [functions?] were not more or less in a morbid state; there never was a concurrence of this morbid state, in this complete circle of organs, without fever. The events which *invariably* concur in fever, then, are a certain deviation from the healthy state in the nervous and the sensorial functions; a certain deviation from the healthy state in the circulating function; a certain deviation from the healthy state in the functions of secretion and excretion. A deviation from the healthy state in one circle of action will not present the phenomena of fever; a deviation from the healthy state in two circles of action will not present the phenomena of fever; there must be a deviation in the three circles before fever can exist. Such, then, are the common phenomena of fever." P. 60.

These copious extracts give our readers a full view of the

doctrine of fever advocated by Dr Smith. He, however, does not think that the mere invariable concurrence of morbid action in the three systems mentioned will, of itself, constitute fever: their derangement must occur in an invariable order. And here the author evidently proceeds on the principle, that the identity of a morbid affection is not determined merely by the organs or functions affected being identical, but also by their being affected in the same order. We agree with the author on this point, that the sequence of morbid phenomena is an essential character in disease. What, then, is the sequence of morbid affection in the three systems mentioned, and is it in an invariable order? Dr Smith answers this question in the affirmative, and asserts that the derangement takes place first in the nervous and sensorial functions; next in the circulating function, and last in the secreting and excreting functions.

The author next examines the question,—*Is fever inflammation?* In his opinion, it is not. In fever there are certain events, occurring always in the same order; and the same is true, according to him, in regard to inflammation. But in inflammation the events are different; or if this point is not so well made out, at least, the morbid events occur in a different order. Thus in diseases admitted to be inflammations by all parties, such as pneumonia, hepatitis, &c., the spinal cord and brain are *never* the organs in which the *first* indications of disease occur. On the contrary, the earliest indications of disease are afforded by the organ which is the seat of the inflammation; and if the nervous function is affected, it suffers but little, and is the last to be involved, instead of the first, in the series. The author very justly remarks, that we do not know what is the physical condition of the organs involved, whether in fever or inflammation, contrasted with their state in health, and perhaps we shall never arrive at this knowledge. Thus he says—

"What inflammation is beyond the series of events we are able to observe, we do not know; what fever is beyond the series of events we are able to observe, we do not know. We compare the events and we see that they differ; and since the use of names is to mark and express differences, it is right to distinguish those different events by different terms. But though in the present state of our knowledge, we are not jus-

tified in considering fever and inflammation to be the same; yet the close, perhaps the constant connexion between them, is a fact of the utmost importance to be known, and requires to be incessantly before the view of the practitioner." P. 64.

The author, believing that he has established what are the functions deranged in fever, and their invariable order of occurrence, next proceeds to show how the infinite varieties of fever may be accounted for, consistently with his doctrine. This he does by adverting to the fact of the different degrees to which the several functions involved will be affected. Thus, says he, if the spinal cord and brain be intensely affected in the outset, violent pains in the limbs, ferocious headach, early and violent delirium will occur; or, on the contrary, there may be loss of voluntary motion, and such an overwhelming of the sensorial faculties, as almost to constitute apoplexy. Here then we would have the symptoms which usher in the plague. If the organs of secretion are seized with peculiar violence, the liver may pour out an immense quantity of vitiated bile, which may irritate every surface it touches, and rapidly tinge the whole body. In this case, according to the author's pathology, we should have the yellow fever of the West Indies. If there should be less violent affection of the spinal cord and brain than occurs in plague; if there be great pain in the back and limbs, intense headach, early and violent delirium, a burning skin, a quick and strong pulse, urgent thirst, and constipated bowels; we shall have an example of the acute, cerebral variety of fever of Great Britain. If there be, not headach, but giddiness; not delirium, but stupor; not a burning hot, but a moderately warm or cool skin; not a frequent and strong, but a frequent and feeble pulse, then we have the subacute cerebral variety of fever of the same country. If with symptoms similar to the last mentioned, occurring in the commencement, there be associated greater prostration, and a rapid increase of the derangement of the nervous and sensorial functions, together with a brown and dry tongue and tender abdomen, and dark and offensive stools, we have what is called typhus.

In the examples here given, the more urgent symptoms occur in one set only of the organs which compose the circle involved in fever; nevertheless, symptoms are present, accord-

ing to the author, indicative of derangement in the two other sets of organs.

Dr Smith very properly criticises the attempt to seek for the common phenomena of fever in the symptoms, in the following words:

"That no two cases of fever can ever be precisely the same, and that it must be vain to seek for the common phenomena of the disease in the external symptoms must now be obvious; and why success can never attend the search after these common phenomena in such symptoms as 'shivering, frequent pulse, heat,' must be equally manifest. These as well as all other symptoms depend upon the state of the organs. But we have seen, that in one degree of the same affection of the same series of organs, there may be shivering, excited pulse, burning heat; while in another degree there may be no shivering, a slow pulse, and a cold skin: so that from one and the same affection, differing only in the degree of its intensity, the symptoms may not only vary, but be directly opposite. The proper object of pursuit in all these inquiries, therefore, is the real nature of the affection, and the symptoms are of consequence only as they are indications of the existence of that affection. Symptoms are not *the* thing in which observation should terminate, but signs of the thing, without the knowledge of which, in every individual case that may come under his care, the practitioner ought never to be at rest, and to the discovery of which they serve as guides." P. 70.

The author objects to the common division of febrile diseases into idiopathic and symptomatic. He objects to it because he considers the so called symptomatic fevers to be inflammations. According to his view, there are no fevers but idiopathic fevers. He also criticises the division of true or idiopathic fevers into intermittent and continued, as a classification founded on a single fact in the highest degree trivial. We subscribe most heartily to this sentiment of the author.

Again, the division of continued fever into synocha, typhus, and synochus, is liable to the objection that it is erected upon no assignable principle. The phenomena, laid down in the nosologies as constituting synocha, exist in all acute inflammations; and the other two names are liable, according to the author, to this further objection, that the groups of symptoms said to characterize them do not occur in nature.

"Again, while, according to this received arrangement, a train of symptoms, every one of which is found in acute in-

flammation, is made a distinct genus of fever, numerous diseases, each forming an exquisite specimen of fever, are totally excluded from the order, and placed at a considerable distance in the nosology. Because scarlatina is a fever attended with a peculiar eruption on the skin; because rubeola is a fever attended with an eruption on the skin also peculiar; because variola is a fever with another peculiar eruption, and urticaria with another; these diseases are not made varieties of fever, but, designated by the term exanthemata, are formed into a separate order: while, on the other hand, fevers attended with petechiæ, with papulæ, with aphthæ, with vesicles, are accounted fevers, and accordingly are termed petechial, miliary, aphthous, erysipelatous, vesicular fevers; whence synochus petechialis, synochus miliaris, synochus aphthosus, &c." P. 76.

Dr Smith does not object to the propriety of naming these varieties of disease, but to this mode of classifying them, which diverts the mind from dwelling on those circumstances which make all of them varieties of one great disease, and fixes it upon those comparatively unimportant circumstances, which simply modify the malady, without in the least affecting its identity.

The author lays it down as a principle that the formation of the species of fever cannot be scientifically accomplished, until we have arrived at a perfect knowledge of the conditions of the organs on which the different trains of symptoms depend; and that as this exact knowledge has not been attained in regard to many of the species, the classification cannot possibly be made at present. Thus we do not know whether the condition of the organs is the same or different in intermittent and in continued fever; but this we do know, that a distinction founded upon mere periodicity is a very unsatisfactory principle of arrangement. We also know that intermittent and continued fevers are prone to pass into each other, in such a way as to establish an identity in their essential nature. What may be the cause of periodicity in fever is a point, according to Dr Smith, enveloped in doubt: thus he says,

"What that condition of the system is, which, in forms of fever that are thus mutually convertible, causes one to persist in an uninterrupted series, another to remit, and another, after disappearing for a time, to recur in distant and regular paroxysms, is wholly unknown. Sydenham, indeed, cuts the knot and removes the difficulty at once. Speaking of the re-

turn of the fits in intermittent, he replies to the inquirer into their cause,—'I would fain know why a horse comes to his growth in seven years and a man at twenty-one; or why some plants flower in May and some in June. I am persuaded that the progress of nature is as certain and regular in this case as in any other, and that the matter of a quartan and tertian ague is subject to nature's laws and governed by them, as well as any other bodies whatever.' The regularity of nature in the production of disease, no less than in the maintenance of health, cannot be doubted; but the point in question is not clearly one of those ultimate facts, into the reason of which it is wholly vain for the human mind to inquire." P. 80.

On the question, what are the pathological differences between intermittent and continued fever, the author makes the following remarks:

"Hitherto, no one appears to have hazarded, even a conjecture, as to the cause of this striking difference between these two forms of disease; and pathology has afforded no clear light to enable us to determine whether the febrile circle of organs is similarly affected in both. Examinations of fatal cases have been made, but none on that large scale and with that accuracy which alone can render them of any value. I have endeavoured to ascertain the morbid appearances in the spinal cord and the brain, and in the mucous membrane of the respiratory and digestive apparatus, from those who have been long engaged in extensive practice in districts in which the ague prevails, but I have been able to obtain no satisfactory answer, excepting that intermittent does not kill! Greatly as the severity of intermittent is, without doubt, diminished in the present age, yet we cannot receive such an account without blessing the bark of the seventeenth and the skill of the nineteenth century." P. 81.

Dr Smith states that the chief object of his treatise is to determine the pathological condition of the organs in the fevers which prevail in London, and which his situation as physician to the London Fever Hospital gives him such ample opportunities of investigating. It is only fevers called continued that he proposes to notice, and before entering upon them, he discusses the questions of the true principle of arrangement of fevers, and the nomenclature most convenient to be employed.

Dr Smith lays it down as a pathological canon that the same assemblage of symptoms in fever is always accompanied by the

same invariable condition of the organs; that this condition of the organs in fever is always the same in nature, only differing in intensity in different cases; and that, therefore, fever is a unit, and differs, in its different forms, only in the intensity of the affection. We have, therefore, nothing to designate by names in fever, but different degrees of intensity. Some of these degrees it would be frivolous to distinguish by a name; while others, being practically important, must be designated by particular appellations. The author thinks all useful and practical purposes will be answered by arranging continued fevers under two great classes, the one comprehending the mild, the other the severe forms of the disease. As, however, he is averse to introducing new terms, he adopts, in preference, two names in common use to denote these classes:—namely, *synochus*, to express the milder forms of fever, and *typhus*, the severer forms, without reference to any particular meaning given to them by Cullen or other writers. To distinguish further differences which the author thinks have a bearing on practice, he subdivides synochus and typhus severally into *mitior* and *gravior*. These divisions he conceives will afford convenient and ample means of throwing into distinct groups all the varieties of fever that occur in Great Britain, which it can be of any practical importance to distinguish.

The author thinks this plan of nomenclature might be applied with advantage to fevers attended with eruptions. Thus scarlet fever and small pox each occur in a mild and severe form; and hence we would have scarlatina synochodes and typhodes; and variola synochodes and typhodes, &c.

CHAPTER III. *Synochus. Phenomena in Synochus Mitior. Phenomena of Recovery. Transition of Synochus Mitior into Synochus Gravior. Classification according to the different Organs in which the several Affections have their Seat.*

The symptoms in synochus mitior are, 1. Loss of mental energy. 2. An uneasy sensation, quite peculiar, which is different from, and more distressing than pain, and which Dr Smith proposes to call febrile uneasiness. 3. Pain first in the back and loins and then in the limbs. 4. An expression of dejection in the countenance, with a pallid and shrunk

appearance of the features. 5. A sense of chilliness, which, being independent of any diminution of caloric, indicates an altered sensibility in the skin.

All these symptoms are considered by the author as indicative of derangement of function in the spinal cord and brain. About this time the circulating system is beginning to be affected, evinced by the altered state of the pulse, which is invariably weaker than in health. At the same time, the respiration is shorter and quicker than natural. After a shorter or longer interval, the pulse, which was feebler than natural, becomes more full, strong, and generally more quick than in the sound state. Now the skin becomes really preternaturally hot, as evinced as well by sensation as by the application of the thermometer; but this heat is not uniformly diffused, being more intense in certain parts, as for example the head, than in others. Next the third order of functions in the febrile circle becomes deranged; that is, those of secretion and excretion. The mouth is now dry and parched, and the tongue begins to be covered with fur. The secretions of the liver, and of the mucous membrane lining the whole alimentary canal are vitiated, as is proved by the unnatural quantity, colour, and fetor of the evacuations. The urine is likewise altered in its appearance, and the skin becomes dry and harsh as well as hot. At this period the pain in the back and limbs, and the general febrile uneasiness are much augmented.

We have given above the symptoms of synochus mitior as detailed by Dr Smith, from the first deviation from health to the last symptom in the series; and it is perceived that he makes out his case very satisfactorily. For while his description of the given form of fever is accurate, there is clearly shown to be a deviation from health successively in the nervous and sensorial functions, in the circulating function, and in the secreting and excreting functions.

The ordinary fever of London at the present day is characterized by an uninterrupted continuity in the phenomena, there being nothing like remission or intermission throughout its whole course. The tongue, in the milder form of the disease, remains moist and never becomes brown. Almost all attacked with this form begin to recover about the end of the second week. But now and then the symptoms will be mild

for the first two weeks, and then, instead of convalescence, there will be no perceptible improvement. The patient grows weaker and lies more prostrate. Still he complains of no pain or uneasiness, and it is not easy to detect any trace of disease in any organ; yet it is but too evident that he will sink exhausted. After death it is commonly found that disease has been preying on some vital organ, although its presence could not be detected during life. Such cases generally occur in aged persons of worn out constitutions.

In this form of fever the first indication of returning health is marked by longer and more tranquil sleep. The patient lies more still, and on waking from his more tranquil slumber, spontaneously says that he feels better. The countenance becomes more animated, and its natural expression returns; the tongue begins to clean; and, after an interval of a few days, the appetite returns. In the mean time, the pulse usually becomes slower about ten beats in a minute; though occasionally it remains frequent throughout the entire period of convalescence.

"In some cases, on the contrary, when the attack has been very mild, the pulse sinks considerably below the natural standard, and is intermittent, a sign which I have uniformly observed to be attended with a sure and steady convalescence. In the meantime, the appetite becomes keener than natural; the strength gradually improves; and in a short time the patient is restored to his usual health and vigour." P. 104.

In the synochus mitior, we have no opportunity of knowing the state of the organs from actual inspection, as the disease terminates favourably with hardly an exception; but Dr Smith very fairly infers that the morbid affection of the organs is slight, and that the diseased process which it sets up in them stops before it produces any change of structure.

When a mild form of fever, such as is above described, becomes severe, that is, passes into synochus gravior, it becomes so in consequence of the supervention of inflammation in certain organs. But Dr Smith contends that the inflammatory action thus supervening is considerably modified by the febrile state. The author has taken much pains to note the symptoms in these severer forms of fever, and to connect them with the several morbid appearances of which they are the signs. He has convinced himself that the organs secondarily

affected in these severer forms of fever are *always the same*, that the symptoms which indicate the morbid changes in them are *uniform*. The organs affected are the spinal cord and brain, with their membranes, the mucous membrane of the lungs, and the mucous membrane of the intestines, or in other words, the cerebral, thoracic, and abdominal organs. Each of these three sets of organs may, in different cases, be affected prominently, or they may appear to be equally involved. On all these four suppositions the symptoms will be peculiar and invariable; and, therefore, Dr Smith has thought it proper to describe Synochus Gravior under four heads: I. Synochus Gravior with Cerebral Affection, subdivided into subacute and acute. II. Synochus Gravior with Thoracic Affection. III. Synochus Gravior with Abdominal Affection. IV. Synochus Gravior with Mixed Affection.

I. Synochus Gravior with Cerebral Affection.

1. *Synochus Gravior with subacute Cerebral Affection* is attended with all the symptoms of synochus mitior up to a certain period, when the pain in the head, instead of diminishing, increases. The pain is not severe, and need not be, to indicate, nevertheless, extensive and dangerous affection of the brain. When pain is absent, it is replaced by giddiness; and where the two alternate, it indicates a more severe affection than where either exists alone. The heat of the skin is not generally great, but the head is usually hotter than other parts, and hottest over the seat of pain. The eye has a dull and heavy expression; more so than in the milder form of fever. The conjunctiva generally becomes bright and more glistening than natural; though, instead of this, the vessels are often turgid, and give the eye a "muddled" appearance. There is great sensibility to light and sounds. The countenance has a peculiar expression not easy to be described. The face is often flushed, but sometimes pallid. There is much restlessness, and no refreshing sleep. Delirium of a mild character now generally comes on. In the mean time, the pulse, tongue, and evacuations do not materially differ from their state in the mild form of the fever, or synochus mitior.

Such is the train of symptoms which Dr Smith enumerates as indicating a subacute affection of the brain. They generally last from eight to fifteen days, when a remarkable change

takes place, which, according to its character, indicates recovery or a fatal issue.

If recovery is to take place the patient enjoys refreshing sleep, which relieves the febrile uneasiness, headach, and heat and dryness of the skin. The next sleep is longer and more refreshing, and after waking, the countenance becomes more natural, the skin cooler, the tongue cleaner, and the pulse slower. From this period convalescence will probably proceed just as in synochus mitior.

If the case is to be fatal, the pain in the head, at this period, disappears or strikingly diminishes. The pain in the back and loins, however, will frequently continue for some time longer. Simultaneously with these changes, there is a remarkable diminution of sensibility. "The mind is duller and more heavy. The patient may still be roused to answer with tolerable coherence if spoken to; but when left to himself he is confused and stupid. The eyes now become injected, often suffused; and the heaviness and dulness of their expression is increased."

At this time, delirium most commonly comes on; or, if the increased insensibility be not attended with delirium, it is almost always accompanied with moaning and incoherent muttering. The pulse, in the mean time, becomes more frequent and weaker. Symptoms indicative of the implication of the chest and abdomen now arise; as a case purely cerebral rarely happens. The tongue becomes loaded and often dry; "and it is strikingly characteristic of the state of the nervous system, that while the tongue becomes dry the thirst diminishes."

The disease may proceed thus far, and yet, according to the author, not prove mortal. When recovery is to take place, more tranquil and longer continued sleep is always the earliest sign. Other favourable signs are:—less delirium on awaking; increase of sensibility, or improvement in the countenance, even though slight; and the tongue cleaning on the edges, or becoming moist. These favourable signs concurring, recovery may be predicted with tolerable certainty. It may be two or three days before the pulse changes, but generally it will fall ten beats in a minute after the lapse of twenty-four hours.

"Now and then, on leaving a patient in the evening with a

pulse at a hundred and twenty, we are surprised and delighted to find it in the morning as low as a hundred. When the pulse has thus fallen towards the natural standard, when the tongue has begun to clean, and the skin has become cool and soft, however desperate his condition but a few days before, the patient may be said to be convalescent.

"But though this favourable change is sometimes witnessed, yet, from the point at which we left off the description of the progress to a fatal termination, the too common history is increasing restlessness and sleeplessness; insensibility lapsing into coma; further acceleration of the pulse; greater dryness of the tongue and decreasing strength; until, at length, the powers of life, receiving less and less supply from the great system in which they have their seat, become completely exhausted." P. 118.

The author next goes on to enumerate other trains of symptoms which occur in particular cases, but which we have not space to notice.

2. *Synochus Gravior with acute Cerebral Affection.*—When the cerebral affection is acute, the symptoms are the same as in the subacute form, only more intense. The pain in the head, back, loins and limbs is more severe, and the giddiness more violent. The sensibility is excessive, and the least noise or light becomes intolerable. The skin is intensely hot, but peculiarly so over the scalp; so that if the head be shaved and wet cloths applied, they become quickly dry. The febrile uneasiness is excessive, the patient being very restless and wholly without sleep. The pulse at one time is strong, full, bounding, and not easily compressed, but almost always soft; at another time it is oppressed. A slow and intermitting pulse is an alarming symptom, as it is characteristic of an exceedingly acute attack of cerebral disease.

If remedies are not vigorously employed the pain ceases, and passes into insensibility within the fourth day. The indications of thoracic and abdominal affection are less marked than in synochus with subacute cerebral affection; but this arises from the intensity of the cerebral affection, which obscures the signs of disease in the other organs.

Such is an outline of the symptoms, as given by the author, characterizing synochus gravior with cerebral affection, whether subacute or acute. The former runs its course generally

in from three to six weeks, the latter in from seven to ten days.

II. Synochus Gravior with Thoracic Affection.

Perhaps in all cases of febrile disease the mucous membrane of the bronchia is affected; but when the thoracic affection becomes prominent, the symptoms properly constituting the febrile train are modified, and others are superadded derived from the disease of the respiratory organs.

The new and peculiar symptoms in this modification are, pain in the chest, sense of stricture or dispnœa; inability to expand the chest by a full inspiration, without pain and uneasiness; cough, sometimes dry, sometimes accompanied with frothy mucous expectoration; respiration slow and heavy, or short and quick, but never natural; being frequently accompanied with the noise termed mucous rattle. The pulse in the commencement may not be above 80 or 90. It is generally weak, though now and then full and of good strength; but whatever other character it may possess, it is almost always soft. As the disease advances it rises in frequency and becomes weaker. Towards the end of the disease it is almost always hurried and feeble, although occasionally it is observed at this period to become suddenly slow and intermittent. The tongue is unusually foul, commonly moist; but, in severe affections, and in their advanced stage, it sometimes becomes dry. The skin is often moderately warm, but never intensely hot. Commonly it is cool, and of a more dusky colour than natural.

III. Synochus Gravior with Abdominal Affection.

In synochus mitior the affection of the mucous membrane of the stomach and bowels is slight, and passes away without producing any change in its structure. Indeed Dr Smith contends for the invariable derangement in the functions of this organ, throughout its whole extent, from the mouth to the anus; but he does not seem to consider this derangement as primary or causative, as the Broussaists would contend, but coexistent with the febrile train, or consequential to it. Thus in characterizing this division of synochus, he says the abdominal affection may be severe from the commencement, and give early and obvious indications of its existence; or it may come on at some subsequent stage of the disease; and then, although the

affection be equally severe, the symptoms which denote it are materially different. On the basis of whether the abdominal affection occurs early or arises late in the disease, the author describes synochus with abdominal affection under two heads.

1. If the abdominal affection be severe from the commencement, there will be, in addition to the ordinary symptoms of fever, nausea, sometimes retching, and at other times vomiting. Whenever they occur in the commencement they are to be taken as certain signs of abdominal affection, more severe than ordinary. At this early period the bowels are commonly constipated, aud have been so for some days previously to the attack; but in a day or two they pass into the opposite extreme. Dr Smith considers the concurrence of nausea, retching, vomiting, and purging, in the commencement of fever, to be a certain proof that severe abdominal affection is present, which, if not actively treated and effectually checked at this early stage, will soon render the case formidable, if not hopeless.

When the abdominal affection is thus early and open, it is often attended with pain, which, with the other symptoms mentioned, characterize exquisitely marked cases; but pain is by no means an invariable symptom even in severe cases. Pain, upon pressure on the epigastrium, is much more uniformly present. After the tenth day of the disease, at a period when the abdominal affection is most intense, the patient rarely complains of pain, even though it may have been severe at first; nevertheless, the slightest pressure on the abdomen, but especially on the epigastrium, cannot be borne. Thus, though acutely sensible to pain on the least pressure, he is wholly unconscious of it when left to himself.

Dr Smith here remarks what he considers a remarkable coincidence in the progress of the symptoms in the cerebral and abdominal cases. In both, the pain in the head and abdomen respectively diminishes after a certain time, and then ceases; and he conjectures that the analogy would be complete, if the brain, like the abdomen, could be examined by pressure.

"While the pain lessens or ceases as the abdominal affection advances, the purging, on the other hand, continues, often it increases. Purging, succeeding to constipation and to pain, and remaining after the subsidence of the pain, affords an infallible indication of abdominal disease." P. 144.

In these abdominal cases, the tongue is preternaturally red. Sometimes this redness is bright and vivid, and pervades the whole tongue; at others it is confined to the edges or tip. In the latter case, the body is often loaded with fur, which is variously of a dirty white, grayish, or dirty yellow colour. The papillæ are much enlarged, and are seen prominent through the fur, vividly red. In this condition the tongue always remains moist for some time, and the thirst is not urgent; but as the intestinal disease advances, it becomes less vividly red and more dry, and, in the meantime, the lips and teeth often become sordid.

"Instead of being from the commencement of a vivid redness, the colour of the tongue, in other cases, is of a darker and duller tint; there is less fur upon the body, and that which covers it is of a dirtier and darker tinge. This state of the tongue is always attended with greater thirst; it is apt to become more and sooner dry, and, at the same time, the lips and teeth become more and sooner sordid." P. 145.

The author states that the pulse in this form of fever is without any striking or *apparently* distinctive character.

We have already stated the view of the author, that while the pain in the abdomen ceases, the purging continues or increases; but at a still more advanced period, the purging, he contends, also disappears, and the stools return to a more natural condition. These conditions of cessation of pain and return to more natural stools, *if accompanied by corresponding amendment in the other symptoms*, may be hailed as signs of returning health, but otherwise they merely indicate the transition of one diseased process into another. Thus it is perceived that in this subdivision of febrile disease, the succession of events, in regard to the evacuations, is first constipation, then purging, and next the cessation of purging and return to more natural stools.

2. The author next describes the subdivision of synochus with abdominal affection, where this arises in the progress of the disease, in the following graphical manner.

"The second form under which the abdominal affection exists is attended with much less striking symptoms. It requires great attention and daily examination to discover its presence and to trace its progress. It steals along its fatal course with a step as silent as it is sure, and the destruction that marks

its track is oftentimes alike unfelt by its victim and undiscovered by his most watchful guardian. It does not attack until the sensibility is already greatly diminished in consequence of the progress of febrile disease. No pain is therefore felt, and the only indication by which it can be detected is tenderness of the abdomen on pressure. But even the fullest pressure, although it generally excites some uneasiness, sometimes produces none whatever. There is often no purging, for when the affection comes on thus late, though the bowels may sometimes be loose, yet they are frequently even constipated. The tongue is generally red at the edges and the tip loaded with dirty gray or yellow fur, and sometimes dry. The pulse at this advanced period is generally 120." P. 147.

"On recovering from this state, for recovery does sometimes take place, the first indication of improvement commonly appears in the tongue, which shows a disposition to clean; and what is remarkable, the favourable sign which accompanies this improved condition of the tongue is *increased tenderness of the abdomen on pressure.* Not that the disease in the intestine is increasing, but disease in the brain is lessening, and, therefore, the patient is now sensible to a stimulus which before produced not the slightest impression. If on the following days the tongue continue to clear; if it grow less red; if at the same time the pulse fall, the sleep return, the sensibility increase, and the countenance become more animated, the patient may be considered convalescent." P. 148.

IV. Synochus Gravior with Mixed Affection.

In treating of this division of synochus, the author cautions his reader against supposing that the epithet "mixed affection" is to be taken as implying that the cases previously described were unmixed. All that he wishes to convey by this designation is not simply that the organs of the brain, thorax and abdomen are affected, but that they are all affected in an exquisite degree. These cases must, therefore, be the most dangerous with which the physician has to contend.

Cases of this description occur much oftener than from the symptoms we should be led to suppose. Examination after death discloses their true nature. The brain, lungs, and abdomen are often found most extensively diseased, while the indications were confined, perhaps, to the brain and abdomen, or to the brain and lungs.

In these mixed cases Dr Smith contends for the sameness

of the affection in each organ, as when one organ only is affected. He conceives that each organ is liable to its own specific disease, and that the disease in it goes on with the utmost regularity, whether it be the sole organ so far diseased as to suffer a change in its structure or not. At the same time, he contends that the symptoms arising from each organ are essentially the same in the two cases. These views of the author may possibly be correct, but they have not struck us to be well founded.

From the view which the author takes of the subject, he conceives that it would be a mere repetition to detail the symptoms in the mixed cases, and therefore refers the reader, for the modifications they undergo in particular combinations, to the cases.

Near the close of his account of the mixed cases, the author expresses himself in the following manner:

"It seems to me to be impossible to study the pathology of 'the mixed cases,' without premising that the opinion that the seat of fever is invariably in some *one* organ is founded in partial, and, therefore, imperfect views; and I earnestly solicit the attention of those, who have hitherto contended for the strict locality of that seat, to these very interesting and instructive cases. It was by slow degrees, and after the study of the symptoms as they occur in all varieties, and, if I may so speak, shades of type, *in connexion with the morbid changes apparent after death*, that I was able to make out, what I have so often stated to be, the true circle of organs upon which the disease always seizes and always preys, and which it often irreparably destroys. In some of these mixed cases we see marks of irreparable destruction in this entire series of organs; and in every one, we see extensive disease in all of them. Coupling then, as we ought always to couple, these ascertainable and ascertained conditions of the organs in the fatal cases, with the symptoms of derangement manifested by these organs in *all* cases, whether fatal or not, a body of evidence presents itself, which appears to me to be irresistible, to justify the conclusion that the local seat of fever is at least co-extensive with these organs." P. 157.

In the above extract our readers are presented with a general view of the difference in doctrine in regard to fever between the author and the followers of Broussais.

CHAPTER IV. *Typhus. Division into Mitior and Gravior. Typhus Mitior with Cerebral Affection, Subacute—Acute; with Thoracic Affection; with Abdominal Affection. Typhus Gravior. Dangerous Error that it consists in Debility.*

Though the appearance of a person labouring under typhus is different from that of a person affected with synochus, yet Dr Smith considers that a careful examination of the symptoms and morbid appearances in the two diseases clearly shows that they are really identical, and only different intensities of the same affection.

Typhus, like synochus, is divided by Dr Smith into mitior and gravior, and these divisions are subdivided, according as the brunt of the disease is borne by the brain, lungs, or intestines.

TYPHUS MITIOR.—I. TYPHUS MITIOR, WITH CEREBRAL AFFECTION.

This occurs in two degrees of intensity, distinguished by the author as subacute and acute.

1. *Typhus Mitior, with Subacute Cerebral Affection*, presents generally the same symptoms with the corresponding affection in synochus. The difference consists in certain modifications which the symptoms undergo, and which are detailed by the author as follows:—

1st. There is greater prostration of strength, both physical and mental, as evinced by great muscular debility, and by the great contrast often presented between the vigour and torpor of the mind in the course of only a few hours.

2d. The chilliness is greater and longer continued than in synochus; yet shivering is less constant, and the succeeding heat is seldom as great.

3d. The febrile uneasiness is greater. The restlessness is incessant; the face is pallid; the features are shrunk; and the expression of the countenance is most peculiar, being indicative of weakness and suffering. The pulse is always weaker and more rapid.

4th. In general, the pain in the head is less acute than in the corresponding stage of synochus. It is often replaced by dulness, confusion, stupor, and giddiness. The eye is dull, heavy, stupid, and without lustre. "It is remarkable, that

while the pain in the head is only slight, the pains in the back, loins, and extremities, and, as the patient himself says, in the bones, are severe."

5th. "When pain is present it diminishes sooner, and disappears more completely than in synochus."

6th. There is little or no sleep. There may be no violent delirium, but there is much inquietude.

7th. Delirium is more constantly present, and when it occurs, it occurs earlier, as on the sixth or seventh day. It consists in low muttering incoherence rather than in loud and violent talkativeness.

8th. Muscular tremor, like delirium, is much more constantly present in typhus than in synochus; and its relation to delirium is so close, that it is sometimes observed to supervene on the very same day.

9th. In the commencement of typhus, the pulse is sometimes of good strength, and may not exceed 90 in frequency; but as the disease advances it uniformly becomes weaker, smaller, and quicker, so that death rarely takes place before it has reached 120.

10th. The respiration is often not very obviously affected.

11th. The tongue is always foul on the first or second day. It seldom continues moist longer than three or four days, and it is often quite dry as early as the fourth. The apex and edges sometimes remain moist a day or two longer; but in a short time the whole tongue becomes perfectly dry and of a brown colour. As the disease advances, the colour becomes darker and darker, until quite black; it is then frequently fissured into deep chaps, while the lips and teeth soon become covered with a black sordes. "Were the sensibility not greatly altered, such a condition of the mouth and tongue must be attended with insatiable thirst; yet thirst is often not felt."

12th. In the early stage of typhus, the skin is frequently hot; but as the disease advances, it becomes less so, and through the greater portion of the disease it is moderately warm. Towards its termination it becomes cool, and some days before death its temperature falls below the natural standard. It is always of a darker colour than in synochus; the whole surface being of a dull and dusky tinge. Sometimes it is covered with dun-coloured petechiæ, at others, with florid petechiæ.

13th. Erysipelas first appearing on the face, then extending over the scalp, and often down the shoulders and back, is very apt to occur, and is a very unfavourable symptom.

14th. Typhus terminates much earlier, whether favourably or unfavourably, than synochus. Death frequently takes place as early as the tenth or fourteenth day; though by early and appropriate treatment, the force of the disease is sometimes so much lessened that it is as protracted as synochus. In thus attributing to typhus an earlier termination than to synochus, the author evidently restricts his views to primary typhus; for he somewhere admits that all fevers, when long protracted, are prone to assume the symptoms appropriate to typhus.

2. *Typhus Mitior, with Acute Cerebral Affection*, is characterized by the whole train of symptoms just enumerated, they being merely more intense in degree.

II. Typhus Mitior with Thoracic Affection.

We find ourselves altogether unable to condense the author's graphic description of this variety of typhus, and therefore give it in his own words:

"Prominent thoracic affection, as we have seen, is not unfrequent in synochus; in typhus it is more constant; and the signs which denote its existence are more obvious, but they are not precisely the same. The pain in the chest is less severe; it is more often absent altogether; while the sense of stricture and the dyspnœa are more urgent. The cough is more constantly attended with mucous rattle; the respiration is shorter and more hurried. The skin in general is cooler, and it is always more dusky. The dark colour of the skin, in severe cases becoming quite livid, is one of the most chacteristic marks of intense thoracic affection. The colour of the cheek is at first of a deep and vivid red; as the disease advances, it becomes of a purple tinge, and at length it is quite livid. In these cases it is not uncommon for the respiration to be from forty to fifty in a minute. The pulse is invariably rapid and weak. The cerebral affection is equally peculiar and characteristic; it never consists of intense excitement; it is never accompanied with violent delirium; it is indicated by confusion, and stupor passing rapidly into coma; and it is attended with low muttering incoherence, or disjointed rambling; the trains of ideas that pass through the mind being extremely faint, and linked together by no distinguishable affinity. We know that one of the most essential

conditions to the due exercise of the sensorial faculties is the due supply of the brain with arterial blood; but in this state of the system arterial blood does not and cannot circulate through the brain, because it is not formed in the lungs; the patient is in a state approaching to asphyxia, and in very severe cases he remains for several days in as perfect a state of asphyxia as seems to be compatible with life. Why debility should, in these cases, be carried to the utmost possible extent; why such cases should form the most exquisite specimens of the adynamic state, need not be pointed out: the disease is concentrated in the very organ which elaborates the pabulum of life, and that stream, which should convey its vivifying and animating influence to every nook and point of the system, is corrupted at its source.

"It is in these cases, too, that the tongue becomes dryer than in any other; in its advanced stage it is sometimes quite black and even hard, and is altogether incapable of being protruded. Sometimes it is covered with a thick, black, and hard crust; at other times it is cut into deep fissures, so as to give it a cracked appearance. These states of the tongue without doubt arise in part from the excessive dryness, occasioned by the mouth being kept always open on account of the difficulty of respiring." P. 169 et seq.

III. Typhus Mitior with Abdominal Affection.

This differs but little from the corresponding affection in synochus. The points of difference are the following: in the affection under consideration, pain in the abdomen is scarcely ever felt, and pain on pressure is less acute. More commonly both are entirely absent. On the other hand, the abdomen is more often swollen, hard, tense, and tympanitic, while the stools are more early and more constantly passed involuntarily. Hemorrhage from the bowels most frequently occurs here; an event not very uncommon in the severest and most protracted examples of the disease. The tongue is less constantly red, but more uniformly dry, black, and cracked.

IV. Typhus Mitior with Mixed Affection.

This variety of typhus, strictly speaking, does not exist; for the author says, that "whenever the brain, the lungs, and the intestines, become simultaneously and prominently affected in typhus, the case no longer assumes the mild, but lapses into the severer form. The author accordingly speaks of this complication under typhus gravior.

Typhus Gravior.

Dr Smith asserts, and, considering his opportunities of judging, we confide in his statement, that the typhus gravior of authors is extinct in London. Those cases which approached nearest to it are, according to the author, mixed cases of typhus, or those in which the brain, lungs, and intestines, are simultaneously and intensely affected.

All the examples of this form of fever which the author has seen, he finds referrible to two classes; one in which the arterial action is excessive; the other in which it is oppressed, or rather overwhelmed.

"In the first, the patient lies insensible with delirium, perhaps so violent that he cannot be kept in bed without restraint; with extreme restlessness and constant watchfulness; with rapid and panting respiration; with a tender abdomen, perhaps with frequent and involuntary stools; a dry, black and hard tongue; a quick, yet weak pulse, and the skin universally and pungently hot.

"In the second, he lies insensible, with a cold and dusky skin; with a swollen and livid countenance; with a heavy and oppressed respiration; with a pulse, perhaps not to be felt, or, if distinguishable, either so rapid that it cannot be counted, so small that it is like a thread beneath the finger, and so weak that it is lost by the slightest pressure; or else slow, irregular, and intermittent. In this state the patient is almost as completely paralysed as in apoplexy, and the attack is almost as rapidly fatal as apoplexy. It constitutes what has been called congestive fever." P. 175.

The pages which follow of the author's text contain so many forcible truths, well expressed, that we think we cannot do better than to present them entire to our readers.

"Fortunately, these intense forms of the disease are of rare occurrence: they are witnessed only in solitary instances, and they arise either from exposure to a highly concentrated poison, or from some condition of the constitution, by which that power to resist the influence of noxious agents which is characteristic of life, is more than commonly diminished or exhausted. They have been conceived to form exquisite specimens of diseases of debility. But where is the debility? Not in the disease, for that is of giant strength; not in the patient, for remove, if you can but remove, a part of the load that oppresses him, and instantly an intensity of action will be set up in the whole system, perhaps as great as it is capa-

ble of exerting, and certainly greater than it is capable of sustaining, without the most imminent danger. The brain is overwhelmed by the intensity of its affection; the energy that should animate the system, and of which it is the great source, is withheld; but that energy is suspended, not destroyed; and the debility which seems to be the result is not real, but apparent, not direct, but indirect. The giant that lies prostrate on the earth, mastered by superior power, has still a giant's strength, though he do not at that moment put it forth: give him but the chance of throwing off the load that keeps him down, and he will soon show you that he is not weak. I have always been struck with the extraordinary clearness and decision with which the acuteness of Sydenham enabled him to make this important distinction, perhaps in the very first case that occurred to him, in which the discrimination was required. Having described, in his own powerful manner, an excellent specimen of congestive fever, to which he was called, he states that he ordered the patient to be bled; that the by-standers regarded the suggestion with horror; that the man seemed at the point of death; that to them it appeared that the abstraction of blood must inevitably extinguish the last remaining spark of life; while to him it was manifest that the patient was in this alarming condition, because he was oppressed by an overwhelming load, and if that could only be lessened, his condition would be the very reverse of what it now seemed; that, accordingly, on the removal of some ounces of blood, the state of oppression ceased at once, and fever arose of a true inflammatory nature, for the subdual of which repeated bleedings were required.

"It is remarkable, and it is highly characteristic of these intense forms of disease, that their pathology exhibits a striking contrast to that of the less severe affections. No morbid appearances are visible in the organs which seem capable of accounting for death. There are signs of vascularity; the vessels are turgid with blood, and consequently the organs on which they are spent are in a state of congestion. But they seldom, if ever, exhibit any real appearance of inflammation, and still less do they contain any true inflammatory product. Why? Not on account of debility; but because the force of the disease is so great as to overwhelm the powers of life at the first onset, allowing even of no reaction, and much less of that continued excitement which is part and parcel of the inflammatory state, and which is indispensable to an inflammatory product. Reduce the intensity of the disease a little, bring it just within the limit that is compatible with the continuance of life for a given time, and then the products

of inflammation appear in the greatest possible purity, variety, and extent.

"And this is precisely the fact, as is demonstrated by the condition of the organs in those ordinary types of fever, the essence of which has been supposed to consist in debility, and which have recently assumed the dignified name of adynamic. That men who are capable of looking only at the most obvious appearances of things, who, satisfied with what they find at the surface, give themselves no concern to discover its source, should continue to mistake the effect for the cause, and to consider, as in its own primary and essential nature, *that* to be debility, which is the last result of long-continued and most destructive energy of action, is highly probable; but on that very account, the fallacy is the more deeply to be deplored, because to these men must sometimes be committed the care of human beings who will fall certain victims to the error. It is easy to disregard the voice of reason when opposed to specious, however fallacious appearances, but it is difficult to withstand the evidence of sense. In justification of the strength of the language I use, I, therefore, appeal to the pathology I adduce. The notion of debility in the intense forms of fever, I look upon to be an error no less palpable in its nature than destructive in its consequences; and if the havoc it produces does not confer upon it a pre-eminence as bad as that of the very disease of which it is supposed to constitute the essence, it at least entitles it, in comparison with every other error in medicine, to the distinction recognized in society between the hero and the murderer: the one destroys a single human being now and then, but the other numbers its victims by thousands. It may be difficult to eradicate this mischievous opinion where it was first engendered, and where it still continues to be fostered, in the study of the falsely reasoning theorist, but it is easy to confute it at the table of the pathological anatomist, and it must ultimately fall, if not by the pen, by the scalpel." P. 176 et seq.

CHAPTER V. *Scarlatina. Condition of the Internal Organs the same as in Continued Fever. Events which sometimes occur in Fever, but which are not essential to it.*

The author devotes but five pages to scarlatina, and what he says may be summed up in a few lines. With the exception of the peculiarities derived from the eruption, the condition of the internal organs and the symptoms which denote their disordered state are, according to the author, the same as in continued fever without an eruption.

We have already mentioned the author's division of scarla-

tina into synochodes and typhodes. The former he considers to be generally a trifling malady, and even in its most formidable aspect, capable of being subdued, in ninety cases out of a hundred, by prompt and active treatment. On the other hand, scarlatina typhodes he considers to be a most dangerous malady, being attended with the symptoms of typhus gravior, and susceptible of being subdivided like that disease into a form attended with excessive arterial action, and into a congestive form.

The author has appended to this chapter a page or two on certain events in fever, which he considers not essential to the febrile train, and, therefore, denominates accidental. He thus enumerates them:

1. Sometimes an extreme degree of tenderness takes place over the whole body. The sensibility of the skin is so much increased that the slightest pressure gives pain. This preternatural sensibility indicates a severe form of disease.

2. The author next mentions sloughing sores from pressure. This occurrence is known to the whole profession.

3. In severe and protracted cases, erysipelas is no uncommon visitant. When it occurs, it always indicates formidable internal disease, and is a very unfavourable sign.

4. Pain, swelling, hardness, and suppuration of the glands, such as the parotid, submaxillary, axillary, &c. are not uncommon. Such events show formidable disease, and are very unfavourable signs.

5. Severe pain in the joints, with tumefaction and excessive tenderness on pressure, is another accidental event mentioned by the author, which, when it occurs, usually comes on at the close of exceedingly bad cases, and always proves fatal. This peculiar affection has not, according to Dr Smith, been observed by any other writer.

We have now followed the author through his history of the different forms of fever as they occur in London, according to his classification of them. As our article has been extended to a sufficient length, we must defer our analysis of his chapters on the Pathology, Causes, and Treatment of Fever for appearance in the next number of our Journal.

Art. XIII.—CHERVIN ON YELLOW FEVER.

De l'opinion des Médicines Americaines sur la Contagion ou la non Contagion de la Fievre Jaune, ou Réponse aux allegations de M. M. les Docteurs Hosack et Townsend, de New York, publiées, l'an dernier, dans la Revue Médicale, la Gazette de France et le New York Enquirer. *Par* N. Chervin, D.M.P. 1829, pp. 192.

Examen des Nouvelles opinions de M. le Docteur Lassis, concernant la Fièvre Jaune, ou Reponse à la brochure que ce Médicin vient de publier sur les causes des épidemies en général, et plus particulierement de celle qui a régné, l'an dernier à Gibraltar. *Par* N. Chervin, D.M.P. *Aout.* 1829, *pp.* 46.

Examen des opinions de M. le Docteur Castel touchant la pretendue contagion de la Fièvre Jaune, ou Réponse à un écrit intitulé; De la contagion dans les affections febriles. *Par* N. Chervin, D.M.P. *Juillet* 1830, *pp.* 51.

Lèttre à Mr le Docteur Monfalcon de Lyon, sur la Fièvre Jaune qui à régné à Gibraltar en 1829. *Par* N. Chervin, D.M.P. *Aout* 1830, *pp.* 34.

"*Non verbis, sed factis.*"

In a former number of this Journal, vol. viii, p. 387, &c. we presented to our readers a concise analysis of Dr Chervin's publications on the Yellow Fever. Since that period we have carefully perused several other essays from the same source, and have placed their titles at the head of this article with the intention of devoting a few pages to a summary of their contents.

In our preceding article we stated that a distinguished physician of this country had undertaken, in a letter published in the French newspapers, to invalidate some of Dr Chervin's statements, and that when we last heard from this gentleman he was engaged in writing an answer to that letter. We added, that from all we knew of his talents, and from our conviction of the superiority of his cause, we had no doubt our countryman would have no more reason to be satisfied with

the results of his attack, than some of Dr C.'s opponents in France had with those of their own warfare. The title of the first pamphlet before us will sufficiently indicate that it constitutes the answer we were expecting, and also show to whom we alluded when we spoke of a distinguished countryman. Our prediction of the success of Dr C. in this answer has been fully realised. Indeed, we do not hesitate to affirm, that we have seldom seen so complete and unanswerable a refutation of the charges and assertions preferred against an individual, as the one contained in this pamphlet. Dr Hosack and his friend Dr Townsend will, we have no doubt, think very differently; for no one likes to acknowledge, even to himself, that he has met with a most inglorious defeat. But we, who are not obliged to be so very blind to the errors of these gentlemen as they themselves necessarily are; nor to conceal from ourselves and others that they have behaved unwisely in this controversy, and have been punished for it, must, therefore, be allowed to state freely our opinion; to inform our readers, who will probably see more of the reply, if such a thing can be got up by our New York friends, than they can of Dr Chervin's publication, what was the nature of the impression we experienced from a perusal of the latter, and to guard them against the effect of ex-parte evidence. The writer of this article has not made up his mind on the subject of the controversy simply from reading Dr Chervin's pamphlet. He was already well acquainted with the principal points in discussion, and with the uncandid manner in which Dr Chervin had been treated in New York. He was, moreover, at Paris at the time when Dr Townsend was busily employed there in operating against Dr Chervin, and was a witness to the manner in which he endeavoured to baffle the efforts of Dr C.'s friends; he was in Paris, also, when Dr Hosack's letter appeared in the French papers, and was in almost daily intercourse with several of the gentlemen appealed to as evidence by Dr C. He knows the whole affair, therefore, as well as the parties themselves, and never hesitated, even before reading Dr C.'s pamphlet, to decide in favour of that gentleman, and to regard him in every respect the aggrieved party. We are really sorry that Drs Hosack and Townsend should not have been taught more caution by the constant defeats they

have sustained in this country, than to expose themselves in this manner to the deadly weapons of an adversary, with whom there are but few capable of contending in the field of polemics, in which he has so long been engaged, and who, on this occasion, it must be allowed, has laid on his weapons with no sparing hand. As Americans we blush for the defeat, but at the same time feel little commiseration for those who have incurred it, as they very gratuitously began the controversy, and conducted it with a disregard to the rules of urbanity towards Dr C., and of politeness in the tone of their publications, which reflects little credit upon them, and was quite unexpected in well bred gentlemen. Let us hope, however, that Drs Hosack and Townsend will, ere long, present to the public a satisfactory reply, based upon facts, not assertions, to Dr Chervin's answer; and that Dr T. in particular, will give some good reason for having interfered while at Paris, in an affair which did not interest him personally; for having, in his endeavours to baffle the hopes of Dr Chervin's friends at the institute, or to injure that estimable man in the opinion of the members of the medical academy, represented him in a light different from that in which he is avowedly held by a large number of American physicians, from Maine to Louisiana, and magnified the importance of some reports, and the weight of an authority on the points in debate, which, he ought to know, have made but few converts in this country.

It is right to state for the information of our readers, that at the time the committee of the royal institute was preparing a report to that scientific body, on the Memoirs presented for the Monthyon prize, and when it was yet undecided whether Dr Chervin would obtain the latter, Dr Townsend, who was at Paris, voluntarily went to much trouble to prevent this issue of the *concours*. Notwithstanding his efforts, however, efforts of which Dr C. complains, and, we think, not without reason, as the account of them contained in the pamphlet will convince every one, the institute awarded him a premium of 10,000 francs. Dr T. probably informed Dr Hosack of what was in agitation at Paris, on the subject of the yellow fever, for we find the latter stating to him in a letter which appeared in the *Revue Médicale*, in June 1828, and the *Gazette de France*

in August of the same year, that he is very glad to learn the Academy of Medicine has not pronounced in favour of the non-contagiousness of the yellow fever, as, had it done so, it would have been forced to retract that opinion after reflecting on the facts contained in a letter he had formerly furnished Dr Chervin, and on the publication of which he begs Dr Townsend to insist, as he supposes it not to have been included among the documents presented by Dr C. to the academy. He accuses Dr Chervin of not having behaved in an impartial manner, while examining and extracting from the documents contained in the public archives of New York, and expresses a hope that the institute will suspend its judgment on the ex-parte documents of Dr Chervin, until he (Dr H.) has published a work on the subject, which he confidently expects will lead to very different conclusions from those advocated by Dr Chervin. He adds, that since he gave this letter to Dr C. he has witnessed other facts of the contagious nature of the yellow fever, and has lately received communications of the highest value to the same effect, from Sir Gilbert Blane and others, &c. Dr Chervin shows, in the most conclusive manner, that the letter furnished him by Dr Hosack, during his stay at New York, contains no fact to prove the contagious nature of the yellow fever; that this letter was not suppressed; that he was not as partial as Dr Hosack pretends, while seeking for documents in the public archives of New York, &c. Dr Chervin adds that until Dr H. feels disposed to communicate the additional facts he has collected on the subject of the contagion of yellow fever, we must be allowed to concede little weight in the controversy to the testimony of Sir Gilbert Blane, and proves the necessity of this by an extract from this gentleman's medical logic, in which he states, in reference to the history of opinion on the subject in question, in Philadelphia, precisely the reverse of what is true.

Having disposed of this letter Dr Chervin turns to another attack on the part of Dr Hosack, and to one he had to sustain from Dr Townsend. These attacks were published in the form of letters in the New York Enquirer. This series of documents commenced with a letter addressed from Paris to Dr Hosack by Dr Townsend, dated 13th March 1828. After

stating that the Academy of Medicine was occupied with the question of the contagion of yellow fever and with that of the propriety of abolishing quarantine laws, Dr T. adds, "I have been appealed to by the Academy to know how far the allegations of Dr Chervin ought to be depended upon." He remarks, moreover, that regrets are expressed at Paris at his (Dr H.'s) not having published his long expected work on the yellow fever; that Dr Chervin tries to convince every one, that Dr Hosack's silence arises from a sense of the incorrectness of his doctrine—calls this a calumny—presses on Dr Hosack to publish that work—insinuates that Dr C. is one of those *unworthy members of our profession* who, to further their selfish views, are ever ready to sacrifice truth to falsehood, &c. The next letter is from Dr Townsend to the President of the Royal Academy of Medicine, dated New York, 28th November 1828. In this he endeavours to persuade that venerable physician that the Academy of Sciences has done wrong in awarding a premium of ten thousand francs to Dr Chervin. "I did not conceive, he says, that even so small a sum as that would be paid to any person for having spent nearly the half of his life in collecting together, and *afterwards industriously disseminating a mass of spurious papers*, (it is in this way that Dr T. treats documents furnished by the most respectable physicians in this country, Spain, and the West Indies!!) which he knew in his conscience were calculated to present an unfair and imperfect view of the whole ground of the controversy." What will the very great majority of physicians in this country who are non-contagionists think of this? He accuses Dr C. of misrepresenting to the Academy of France the state of opinion in this country on the subject of the yellow fever (!!!); of carefully concealing or suppressing all the facts by which an opinion contrary to his own might be established (how many are there, and in what do they consist?); accuses him of being stimulated *by cupidity more than by patriotism;* states as his belief, that Dr C. was under the influence of preconceived theories and prejudices, before he undertook the investigation of the subject, and that his testimony cannot be relied upon. He talks of a reaction which took place among the members of the Academy after they became acquainted with some facts communicated by Dr Hosack and himself, and when others, re-

ceived from Spain by Mr Parriset, were more generally known, and expresses the belief "that many of the members of that body, as well as of the Academy of Sciences, are now thoroughly convinced that they have lent too credulous an ear to his (Dr C.'s) misrepresentations and malversations; and that they have, in fine, been most egregiously imposed upon." He terminated by once more charging Dr C. with abusing the courtesy shown to him by the Board of Health of New York in permitting him to make extracts from their archives.

This *important document* was presented for inspection to Dr Hosack, who, in a letter of the same date and published at the same time, thanks Dr Townsend for it; speaks of the *base* conduct of Dr Chervin in withholding from the public the communication he (Dr H.) made to him on the subject of yellow fever, and the facts it contains and refers to, calculated to establish the contagious nature of that disease. He reverts once more to the uncandid conduct of Dr C. in *garbling* from the public documents (of N. Y.) those circumstances and materials only which he considered calculated to sustain his belief; to his receiving the reprobation of the municipal authorities of that city; he protests *against the gross imposition* Dr C. has practised upon the liberality of the Academy of Medicine and of Sciences of Paris by his fraudulent misrepresentations relative to Sir Gilbert Blane, to himself (Dr H.) and others, and terminates by stating that the documents possessed by Dr Townsend, derived from the constituted authorities of New York, will most abundantly *convict Dr C. of mala fides* on this subject, so deeply interesting to humanity.

To all this abuse,—to all these grave accusations of dishonest conduct, Dr C. answers in a calm and forcible manner. He refutes in the most conclusive manner every assertion of his opponents, and states a few facts relative to the conduct of one individual while at Paris which reflects little credit upon him. He shows, in reference to the contents of Dr Townsend's letter and the last epistle of Dr Hosack, by documents of an unimpeachable character, that the Academy was *not* occupied with the question of the propriety of abolishing quarantine laws. 2. That the Academy *never* applied to Dr Townsend to know how far the allegations of Dr Chervin ought to be depended upon. 3. That he (Dr C.) never asserted that Dr

Hosack had some doubts relative to the correctness of his doctrine, and that he has never attributed the non-appearance of Dr H.'s work to that cause. 4. That Dr T. is wrong in asserting that he (Dr C.) has profited by the political turn which the question of contagion assumed in consequence of the famous *cordon sanitaire*, to disguise the state of public opinion on that subject in this country; since the *cordon* was established in 1821 and he commenced his researches in 1814. 5. That he was not stimulated by cupidity more than by patriotism, as maintained by Dr Townsend. 6. That he did not suppress, in his communications to the Academy, any of the facts that could militate against his own views; as is proved by the testimony of all the members of the committee. 7. That he did not merely present a *list* of papers; as is shown by the tenor of the report presented to the Academy by that committee. 8. That he did not commence his inquiries with a fixed opinion on the subject of the non-contagiousness of the yellow fever. 9. That the non-contagionists of the United States do not, as maintained by Dr T., differ all more or less in their opinion. 10. That he did not falsely insinuate that quarantine laws have been abolished in this country; since he published so early as 1823 the fact that such laws were in force here. 11. He clears up in the most satisfactory manner the affair of New York, on which Drs Townsend and Hosack have laid so much stress.

In a word we believe that no one can read the publication of Dr C.,—particularly if he has a previous knowledge of the facts, without joining us in the conclusion, that the Paris physician has gained a complete victory over his adversary.

But we must repeat, sorry as we may be as Americans, that such should have been the result of this controversy, we are still more so that Drs Hosack and Townsend should, without any provocation on the part of Dr Chervin, have so gratuitously entered the arena against that gentleman, and drawn down upon themselves so complete a discomfiture. They could hardly expect that, after having been silenced on the subject of the yellow fever in their own country, they would meet with better success abroad, unless they depended on the effects of broad assertion on the minds of those members of both Academies who had not heretofore paid much attention to the

points at issue, and were therefore little acquainted with the *facts*. But, as one of our contagionists could say in more senses than one—*tu l'as voulu George Dandin*. The defeat was of their own seeking; and we do not hesitate to declare, that every time any of our countrymen will meddle in matters that are not their own—every time they will write letters to prove what is not exactly correct—make use of abusive language about any members of the profession or of society at large, who enjoy the esteem of their fellow citizens and of innumerable foreigners—every time they appear unmindful, while abroad, of that self-respect which every individual owes to himself, or of the dignity of the nation to which they belong, and expose themselves, as one of Dr Chervin's antagonists is said to have done, to the reprimands of respectable natives with whom they come in contact; we shall feel little commiseration for them if they receive a severe castigation, be the castigator who he may. Under these circumstances we certainly will not condole with them; all we can do is to exclaim with Molière, *Que diable alloit il faire dans cette maudite Galere*. In the whole affair these gentlemen, and particularly Dr Townsend, appear to have imagined themselves, and to have acted as, the representatives of the medical profession of this country. Against such an idea we solemnly protest, and we beg the Parisian physicians, and the members of the Academy of Medicine and Sciences in particular, should they ever see these lines, to rest assured that Dr Hosack and Dr Townsend, in all their letters, publications and conversations, only expressed their own individual opinions, to which a very few physicians here will feel inclined to respond; and that if we were called upon to choose a representative to be sent abroad, he would very certainly not be selected from among the few contagionists that remain among us.

We have entered so fully on this remarkable little volume, which contains what may justly be called a chef d'œuvre of refutation, that we have little room left for the others whose titles will be found at the head of this article.

In the next publication Dr C. has treated his new adversary with a success equal to that he exhibited over the New York contagionists. Dr Lassis must be admitted to have offered a fine subject for the battering of his herculean antagonist, though we must say, that one circumstance detracts somewhat

from the honour which the latter has reaped on this occasion; —the feebleness of his opponent, and the consequent facility of the victory. In order to show this, and at the same time afford to our readers an idea of the kind of individuals Dr C. has to contend against, and of the nature of their views on the subject of the yellow fever, it will only be necessary to state, that Dr Lassis, who was at Barcelona sometime during the epidemic of 1822, and may be supposed, therefore, to have seen *some* patients labouring under the yellow fever, was at one period a partisan of the doctrine of *infection*, which he even contributed to promulgate by signing the manifesto published at Barcelona in 1822; some years after, and without having had further opportunities of observing the disease, he abandoned that opinion.

Our readers will suppose, probably, that M. L. having discovered the erroneousness of the doctrine of infection, adopted that of contagion; for, on the subject of the origin of the yellow fever, it has generally been found, that physicians who take the trouble to think for themselves, either lean on one side or the other of that question. But M. Lassis, on this point, has avoided the common track, and, giving up one side without embracing the other, thinks that the yellow fever is neither infectious nor contagious. He even goes so far as to maintain that the partisans of both doctrines have done much mischief. This important fact he communicated to the Academy of Sciences on the 10th November 1828, in reference to the fever then prevailing at Gibraltar. He stated at the same time, on his own authority,—and no authority could certainly be better, at least in his estimation, that the fever in question was nothing more than the common fever of Paris, and that to cut short the epidemic, the only thing necessary was to place matters in their natural state; that is to say, to renounce altogether the received system, that of clearing out the infected districts; the effects of which system were, according to him, the sole cause of the calamities the inhabitants were sustaining. The physicians of this country will know how to appreciate, to its just value, this remarkable discovery. Unfortunately for M. Lassis, Dr Chervin, armed with documents of the most conclusive kind, comes forward in the essay before us, and shows, that if such were the truth, the inhabitants

removed from their homes ought to have been the greatest sufferers; while he proves that of four thousand who were thus *cruelly* treated, none, except a few who returned imprudently to the town, had the disease. Dr C. even settles the affair of the young female who was taken sick on the summit of the rock several weeks after her last visit to the town: it was said by the contagionists that her disease was yellow fever, and that she had taken it from a soldier of the garrison; now Dr Chervin proves that the soldier was taken sick fourteen days *after* the girl, who recovered, and, as it would appear, never had the yellow fever. Dr C. cites many other instances besides that of Gibraltar, in which the extension of yellow fever was greatly lessened by the adoption of the plan M. Lassis condemns; and, on the contrary, many others, in which, *things being left in their natural state,* the mortality was excessive.

M. Lassis talks of the superiority of his documents (compiled, we should presume, from works already before the public,) over every other heretofore collected; of the sacrifices he made in going to Barcelona in 1822,—sacrifices which he seems to regard as far greater than those made by Dr Chervin or any one else; of the advantageous results obtained by the application of his principles in various parts, and particularly at Paris, which he thus saved in 1825 from a visitation of plague or yellow fever; of his researches on the subject of this disease, which embrace every fact ancient and modern; of his work on the causes of epidemical diseases, which he regards as the most complete ever written; of his having done every thing; obtained the solution of the problem, &c. M. Lassis talks, we repeat, of all these circumstances in a strain which affords us no very exalted idea of his modesty.

Of the next publication before us we shall say little; not that it is undeserving of attention as a composition, but because Dr Castel, in answer to whom it is addressed, is not entitled to much respect, at least in what regards his opinion relative to the yellow fever. He may be cited as a fine illustration of that class of writers in Europe, who aspire to the honour of contributing to the settlement of a question of high importance, of which they know nothing except through the writings of others. It will be sufficient to state, that Dr Castel, in endeavouring to controvert, in an essay on contagion in febrile

diseases, some of the positions assumed by Dr Chervin, distorts most of the facts which lie in his way, or garbles them before calling them to the aid of his own views. He concludes, from such a collection of mutilated facts, that the yellow fever may become contagious in three different ways; by an increased development of the local causes which gave rise to it; by a peculiar state of predisposition of the individual, and finally, by the emanation that surrounds the patients. Dr Chervin follows him closely on all points, and refutes him in the most complete manner.

The last publication before us is deserving of more general attention, as presenting an *apperçu* of Dr Chervin's observations relative to the yellow fever which prevailed at Gibraltar in 1828. It was not originally the intention of Dr C. to publish this letter, because the report he is preparing on that epidemic will appear soon, and will present a much greater number of details. But Mr Monfalcon, to whom it was addressed, having communicated this letter to Professor Hecker of Berlin, who published a translation of it in his Annals for November 1829; and a truncated, travestied and imperfect version of this translation having been inserted in the number of the *Clinique* of Paris for 1st of May 1830, Dr Chervin was induced to publish the letter entire in several medical journals of Paris as well as in the present form.

We transcribed in our former article a few extracts from some letters addressed by Dr Chervin to one of the editors, in which this gentleman stated that he soon became satisfied that the fever of Gibraltar was identical with that he had observed in the West Indies and in this country: this opinion he reiterates in the publication before us. He states that the disease is the same whether in relation to the individuals it attacks, the mode of its invasion, its symptoms, march, termination, and the method of treatment it calls for. The only difference, he adds, is, that at Gibraltar the disease was less violent and rapid than it is between the tropics or in the southern states of this country. The post mortem examinations also revealed nearly the same morbid phenomena. Dr Chervin next states that all that had been said to establish the opinion of the importation of the disease from the Havana into

Gibraltar on board the Swedish vessel *Le Dygden* is erroneous, and presents an assemblage of absurd, ridiculous and contradictory statements. He presents a clear summary of the causes of the disease, and an exposé of its progress in the place. We have neither time nor space at present, to enlarge on those topics, which will be more properly taken up when we offer a review of the forthcoming Report which Dr C. is preparing. We must, however, be allowed to offer in termination the following extract from the publication before us.

"From the 11th of September to the 24th of December, both inclusive, the number of sick mentioned in the bills of mortality amounts to about five thousand four hundred*; and from the same period to the 26th of December the number of deaths published by the public authorities amounts to about one thousand one hundred and eighty; whilst, according to an account made out daily by persons of veracity, the number of interments during the same period amounted to about one thousand six hundred and sixty. This difference arises doubtless from the circumstance, that many patients not having physicians, no declaration was made when they died. It is necessary, moreover, to deduct from these one thousand six hundred and sixty deaths those that were caused by other diseases.

"Out of three thousand seven hundred and eighty one men composing the garrison of Gibraltar, on the 1st of September 1828, four hundred and thirty-six fell victims to the epidemic, and are included in the number of deaths mentioned above. The mortality would doubtless have been much greater had not the wise precaution been adopted of encamping the troops beyond the western part of the rock, or where the air was contaminated; in other words, out of the infected district. From the moment this measure was adopted, the disease no longer attacked any other of the military than those whose presence the service unfortunately called for in the infected districts, either of the city or of the part of the rock called *the South*. Facts of this kind certainly do not prove the contagious nature of the disease."

* "If to this number we add the cases which occurred before the 11th of September and those which presented themselves after the 24th of December, we shall have a total of more than five thousand five hundred sick.

"We learn from the London Medical Gazette that according to Dr Barry five thousand five hundred and forty-three cases of the epidemical disease occurred from the month of August until Christmas, of which one thousand six hundred and thirty-one terminated fatally. (Note added in July 1830.)"

Dr Chervin continues to have our best wishes and those of innumerable friends in this country*.

Art. XIV.—PHARMACOPŒIA OF THE UNITED STATES.

The Pharmacopœia of the United States of America; by authority of the National Medical Convention held at Washington, A. D. 1830. *John Grigg. Philadelphia,* 1831; *pp.* 298.

The publication of the National Pharmacopœia of 1820 was an event in the medical history of the United States, the importance of which we can now appreciate more truly than at the time of its occurrence. It was then scarcely to be hoped that, in the short space of ten years, a work, marked with so much haste, superficial knowledge, and careless execution, would be supplanted by another characterised by accurate science, competent practical knowledge, extensive research, cautious discrimination and an almost faultless letter-press. The expectation of a revised and improved code, it is true, reconciled the profession in a great measure to the old Pharmacopœia,

* Since writing the above pages we have received from France a pamphlet entitled "*Précis Historique de l'Epidémie de Fièvre jaune qui a régné à Gibraltar pendant l'Automne de* 1828*; par* M. Peter Wilson, *Membre des Colléges Royaux des Chirurgiens de Londres et de Edinbourg; Traduit de l'Anglais et accompagné de Notes, par* N. Chervin, D.M.P.

Mr Wilson's publication originally appeared in the London Lancet, from which it was taken by Dr Chervin. Mr W., at the time of the epidemic, had been practising his profession at Gibraltar for fifteen years, and had occasion to notice the first cases of the disease, both in his private and in his public practice. He is a firm and enlightened defender of the opinion of the local origin and of the non-contagious nature of the yellow fever. He was a member of the commission appointed by the government of the place to investigate the subjects of the origin and nature of the disease, and which was joined with that view to the members of the French commission. Disgusted, however, at the unfair manner in which his colleague, Dr Barry, conducted these investigations, and harassed by the frequent discussions which arose between them from this cause, Mr Wilson retired, and thus left the field to his opponents. His esssay, and the notes and appendix, which Dr Chervin has added to his translation, deserve to be read by those who feel an interest in the philosophy of epidemical diseases.

and induced its general adoption as the standard of the United States. It was justly thought that an imperfect and faulty standard was better than none, where so broad a basis was laid for its future improvement. The glaring errors and omissions, by which that first attempt was distinguished, blinded us, in some degree, to the real merits which it possessed, and which are now made conspicuous by the corrections and improvements it has received.

A principal feature of the American Pharmacopœia is the simplicity of the nomenclature, which appears to us to be founded on just principles. The pharmaceutist is conversant with drugs and medicines in their prepared state. He studies their properties and their sensible characters as objects of trade and commerce, and his art should have its own appropriate terms. The classifications of naturalists, so important to him as fixing their identity, would be but a clumsy and inconvenient substitute for that more simple nomenclature which general use has sanctioned, and which the convention adopted. A comparison between the Pharmacopœias of Edinburgh and the United States will exemplify the correctness of these views.

We cannot do better in relation to this subject than to present the views of the convention, as expressed in the preface of the work before us. The remarks which we quote have relation only to substances of animal or vegetable origin; the chemicals being generally designated, as we conceive, with propriety, by the received scientific names.

"Three different methods of designating these substances have been in use. According to one method, the whole botanical name of the plant, together with the part employed, is expressed in the name of the medicine. Thus, assafetida is called by the Edinburg College *Gummi resina ferulæ assæ fœtidæ*, and jalap, *Radix convolvuli jalapæ*. This nomenclature, though perhaps the most exact, where the botanical characters of the plant are well ascertained, is exceedingly awkward and unwieldy in practice, and is moreover liable to the serious objection, that as the scientific names of medicinal plants, not hitherto thoroughly investigated or accurately described, are frequently changing with the progress of discovery, the pharmaceutic titles must also change, or, if they remain unaltered, must often mislead those not previously well informed.

"A second method, less objectionable than the preceding,

is to designate the medicine by one only of the two names of the plant, in connection with that of the part employed. Thus assafetida and jalap are respectively denominated, in the London Pharmacopœia, *Assafœtidæ Gummi-resina*, and *Jalapæ Radix*. But names formed upon this principle are also frequently of inconvenient length, and must change with the changing views of botanists; and, besides, the same substance is often derived from several distinct species, if not genera of plants, and the appellation is incomplete unless it designate all the sources of the medicine.

"The third method is to adopt one simple title, applicable under all circumstances to the medicine, and, though it may have been originally derived from the name of the plant, not necessarily changing with it; as, for example, *Aloe*, *Senna*, and *Serpentaria*. The advantages of this nomenclature are its brevity, practical facility, and comparative unchangeableness. In the terms *Senna* and *Serpentaria* we have examples of its superiority in this last respect. Senna was originally supposed to be derived from a single plant, the Cassia Senna of Linnæus: but the investigations of botanists have resulted in the discovery of several species which contribute to furnish this medicine; and in the most recent and best botanical works, the Cassia Senna has been separated into distinct species, under distinct specific names, among which that of Senna is not included. The almost insurmountable obstacle which is here presented in the way of the two first mentioned systems of nomenclature is wholly avoided by the last; for the term Senna may and will be used to designate the medicine, whatever changes the names of the plants may undergo from which it is obtained. The root, which is now universally denominated *Serpentaria*, is derived from at least two species of Aristolochia, besides the A. Serpentaria; and as no difference is observable in its appearance or qualities as procured from these different sources, there can be no propriety in multiplying its titles. Such a multiplication, unavoidable in the systems which indicate the plant in the title of its product, is wholly unnecessary when a single definite name is applied directly to the medicine, without any necessary reference to its source.

"The Convention of 1820 very judiciously adopted the plan last mentioned, and designated the great majority of medicines of vegetable origin by a single Latin term, either employed in accordance with previous usage, or derived immediately from the specific or generic appellation of the plant. Thus *Assafœtida*, *Cinchona*, *Opium*, *Quassia*, *&c.*, were terms in general use; *Gillenia*, *Juglans*, *Podophyllum*, *Spigelia*, *&c.*, were the botanical names of the genera; and *Senega*, *Serpen-*

taria, Azederach, and Toxicodendron were those of the species. In some instances it was requisite to depart from this plan in order to avoid ambiguity; as when two medicinal plants of the same genus occurred, the specific names of which could not be appropriated to the medicine, and when two parts of the same plant were considered officinal. It was necessary in these cases, either to adopt an entirely new title, or in the former case to use both names of the plant, and in the latter, one of them in connection with that of the officinal portion. The latter alternative was preferred; and hence the compound titles *Mentha Piperita* and *Mentha Viridis, Stramonii Semen* and *Stramonii Folia.* When, however, a medicine had been long generally known by a name which also designated the genus of the plant, this name was retained without addition, though another medicine, also introduced into the Pharmacopœia, might belong to the same genus—the latter being distinguished by the full botanical title. *Gentiana* and *Gentiana Catesbæi* may be cited as an example of this kind; and *Cantharis* and *Cantharis Vittata* were admitted upon the same principle.

"Though the general plan of nomenclature originally adopted has been retained in the present edition, it will be observed that alterations have been made in particular instances. The Latin names have almost always been expressed in the singular number, even in cases in which, if the English language were considered a guide, propriety might seem to require the plural. Thus *Cantharis, Caryophyllus, Ficus, Galla, Limon, Prunum, Rosa, Uva Passa, &c.*, are used instead of the plural of these terms respectively. The considerations, by which it is thought these changes may be justified, are, that the nomenclature is thus rendered neater and more uniform, and is brought into closer correspondence with the best classical usage. Celsus generally employs the singular number when speaking of any vegetable product as a medicine, though, under other circumstances, it might be designated by the plural. The terms *semen* and *flos* are always used by him as the medicinal titles of seeds and flowers; and the singular has not been introduced into the present work in any one instance, in which either the direct authority of this writer and of Pliny, or very close analogy with their practice, might not be adduced in support of it. The only pertinent examples of the plural to be found in the writings of Celsus are the words *folia* and *baccæ*, which he usually employs to express the leaves and berries of plants when considered medicinally; and his example has been followed in this as in the other case.

"In the adoption of English names, it was not deemed necessary to translate the Latin term literally; and that title was

preferred which custom, and the genius of the language appeared to sanction. Thus *Linum* is not translated into the corresponding English title of the plant, but into *Flaxseed*, which is the well known and universally received name of the medicine. On the same principle, *Fœniculum* is translated *Fennelseed; Ulmus, Slippery Elm Bark; Glycyrrhiza, Liquorice Root*, &c. Nor are the English names always necessarily in the same number with the Latin. Though we may correctly say *Caryophyllus, Galla, Prunum*, and *Rosa* in the Latin language, the genius of our own requires that these terms should be translated *Cloves, Galls, Prunes*, and *Roses*.

"Another peculiarity in the nomenclature of the present Pharmacopœia is the introduction of well known and current synonymes, whenever the officinal title may not from its novelty be well understood, or the medicine which it represents not readily recognised without reference to the name by which it is usually called. Thus the meaning of *Sodii Chloridum, Potassii Iodidum, Liquor Ammoniæ Hydrosulphatis*, and *Ferri Ferrocyanas*, is fixed by the use of the more familiar terms *Sodæ Murias, Potassæ Hydriodas, Ammoniæ Hydrosulphuretum*, and *Ferri Prussias;* and the identity of *Mel Scillæ Compositum* with the remedy popularly known as *Hive-syrup*, and of *Tinctura Rhei et Sennæ* with *Warner's Gout Cordial*, is rendered evident by the introduction of both titles. To prevent misapprehension, the synonymes are distinguished from the proper officinal titles by being printed in a smaller type, and placed in a secondary position.

"While on the subject of nomenclature it may be proper to mention, that, in the Latin titles, the name of the base in every compound chemical term, and that of the plant in those which refer to vegetable products, are placed first in order, both from the impression that such a relative position accords best with the genius of the Latin language, and from the greater facility which it affords in the alphabetical arrangement. Thus *Sodæ Sulphas, Zinci Oxidum, Sodii Chloridum, Colchici Radix, Stramonii Folia*, &c. are employed as here represented, not in the reverse order sometimes adopted. Exceptions to this rule are afforded in the nomenclature of several substances, the titles of which belong to pharmaceutic classes, as the Oils, Extracts, Infusions, Tinctures, &c., in which the name designating the character of the preparation has the precedence; and the same end of facility of alphabetical arrangement is promoted by this deviation from the general plan."

These details may appear somewhat tedious, yet we think they are not beneath the attention of the accomplished physician. Precision and classical purity in prescribing extemporaneous

formulæ are by no means traits of the American physicians, and we should be glad to see them err on the side of fastidious refinement rather than of loose and awkward expression. They would thus greatly advance the interests of pharmacy, by compelling the apothecary to study his business as a science; and the habit of minute accuracy, when once formed, would be found to be of incalculable importance in weightier matters.

It would be doing an injustice to the convention not to quote at length that part of the preface which is devoted to an explanation of the principles which have guided it in the present revision.

"The Materia Medica is arranged in two distinct columns, the first of which presents a catalogue of the standard simple medicines, of the preparations not made by the apothecary, and of all those substances, not strictly medicinal, which are introduced into the formulæ of this Pharmacopœia. The terms employed in this catalogue are, with the exception of a few synonymes, the officinal titles of the substances severally designated, and are used in all other parts of the work where these substances are referred to.

"In the second column are various accessary matters and explanatory observations, intended chiefly to give precision to the officinal terms. Opposite to the name of each medicine of organic origin, is the systematic title of the particular plant or animal from which it is derived, with a reference to some standard authority, and to the part of the plant or animal employed. In giving these systematic titles, the most important botanical and zoological synonymes have also been presented, that which is placed first in order being considered of the highest authority. When the same substance is derived from two or more plants or animals, these are usually indicated; and their names, being connected by the copulative conjunction, may be readily distinguished from such as are intended merely as synonymes. When different opinions are entertained relative to the source of the remedy, the circumstance is expressed by connecting the names of the plants by the disjunctive conjunction. Much care has been taken to render this part of the work as correct as possible, and many errors and omissions existing in the first edition have been corrected or supplied. Strict attention has been paid even to the accurate quoting of authority; and not a single work which was readily attainable has been referred to, without having been previously consulted with a view to such reference."

"The possession of a supplementary catalogue or second-

ary list of medicines is a peculiarity of the old Pharmacopœia, which, as it appears to have had its origin in just views, and is certainly very convenient, has been retained in the present. Its chief advantage is, that it keeps in the view of the profession numerous medicinal subtances, which, though their reputation is not sufficiently established, nor their employment sufficiently general to entitle them to rank with the standard remedies, are nevertheless not altogether unworthy of notice. Such a catalogue may be considered as a place of probation, in which newly proposed remedies may be introduced, and remain till experience shall have established their claims to a higher station, or have proved them utterly worthless; and to which remedies, which have been in general use but have lost their credit, may be consigned to await the judgment of the profession, when the influence of fashion may be no longer exerted against them.

"Every substance, however inefficient in itself, which is employed in any pharmaceutic process, acquires from this circumstance an importance which entitles it to rank with those remedies accounted strictly officinal, and no such substance, therefore, is to be found in the secondary list. This remark is perhaps necessary, as the reader, if not aware of the motives for placing certain substances among the standard medicines, might conclude, that great injustice had been done to many which are stationed in the secondary list, by the preference given to others having apparently much less claim to favourable notice."

"One of the most prominent defects of the original Pharmacopœia is a want of uniformity both in the manner of conducting the processes, and in the style of describing them, arising from the various sources from which the materials were drawn, and the want of due time so to remould and shape them as to enable them to unite together in one harmonious whole. Even the titles do not sufficiently correspond; and the work is altogether deficient in that unity of character, which is especially desirable in every production having the authority of a national standard. An effort has been made to supply these deficiencies, so far as language is concerned, by first adopting one general style of latinity, and then remodelling the phraseology of each process, so as to bring it into conformity with that style. Upon a thorough examination of several Pharmacopœias, and a comparison of each with the best medical writers among the Romans, the work of the London College appeared to approach most nearly to classical purity, and was accordingly adopted as a model in the present revision, though a servile adherence to its authority has been avoided. The Pharmacopœia, having been writ-

ten out in the Latin language, was subsequently translated into English; and it follows, that, if success has been attained in the endeavour to stamp a character of uniformity on the style of expression in the former language, a similar result must have been obtained in the latter. The above remark will account for numerous verbal alterations, which, considered by themselves, without reference to any general plan, might seem to have been made from a desire rather of change than of improvement.

"In the selection of the process for each preparation, the same wish for uniformity has had an influence in producing many alterations. Two principles have governed the choice of formulæ independently of intrinsic merit; which, when superior, has always been allowed a predominant influence. When two or more methods of preparing the same compound, equally meritorious in themselves, have come under consideration, that has been preferred which united in its favour the widest prevalence in this country, and the sanction of the majority of the British Pharmacopœias. These circumstances have often occurred in connexion; for before the publication of the American Pharmacopœia in 1820, those of the British Colleges were the standards generally followed; and it has not unfrequently happened, that when the process adopted by the first American Convention was taken from one of these Pharmacopœias, with the authority of the others against it, the medical and pharmaceutical professions among us have adhered to that most extensively recognized abroad. It is highly desirable that uniformity in the preparation of medicines should every where prevail; for the benefits, accruing from the mutual interchange of the medical writings of different civilized nations, must be greatly affected by any material difference in the nature or composition of the remedies employed. This remark is especially applicable to Great Britain and the United States, and to all countries where the English language is generally used. It is a duty, therefore, which we owe to the cause of pharmacy to throw our own weight into that scale which already preponderates, and thus contribute to the production and maintenance of the desired uniformity. As the London Pharmacopœia is most widely circulated, and exercises the greatest influence, the changes introduced have frequently been in accordance with that work; and it has happened fortunately that merit has usually been on the side of authority. Where the three British Pharmacopœias have been united against our own, without any decided superiority of practical advantage in our favour, no hesitation has been felt in according with the former.

"Many of the changes, which have been made in this edi-

tion, were rendered necessary by pharmaceutic errors in the original. Some of the processes were upon trial found to be impracticable, and many others defective, from a want of due attention to the distinction between weights and measures, in transferring the formulæ of the Edinburgh Pharmacopœia into that of the United States. Other alterations were the necessary consequence of improvements to which the industry and zeal of pharmaceutists, as well in this country as in Europe, have given rise, and of the new light which has been thrown on the science generally since the publication of the first edition. These alterations are interspersed throughout the work, and are too numerous to be noticed individually in this place. Among the processes materially modified, may be mentioned those for Hydrocyanic Acid; Spirit of Nitric Ether; Tartrate of Antimony and Potassa; Camphor, Cinnamon and Mint Waters; Plaster of Carbonate of Lead; Aromatic Syrup of Rhubarb; and Syrup of Sarsaparilla. A few preparations have been omitted, either from the failure of the processes to answer the intended purpose, or from having been superseded by others, analogous in their character, but less objectionable in their mode of formation. *Acetum Purificatum* is an example of the first, *Acetum Opii* of the second. The former is useless; the latter is unscientific in the directions and uncertain in the result, and has been rendered unnecessary by the introduction of the Acetate of Morphia and Acetated Tincture of Opium. Several preparations have also been excluded, because entirely out of use; and some have been transferred to the Materia Medica, as they are almost always procured from the manufacturing chemist, seldom if ever made by the apothecary.

"Within the last ten years several remedies have been introduced into notice, or, if known previously, have acquired a more extensive reputation; and some of them, having been generally adopted by the medical profession, had good claims to a place among the officinal preparations. For these it was necessary to draw up new formulæ, in the preparation of which care was taken to consult the best authorities, and in several instances to submit the processes to the test of actual trial. Among these are the formulæ for Iodide of Potassium, Sulphate of Quinia, and Morphia with its Acetate and Sulphate.

"Finally, with regard to the Preparations it may be remarked, that whatever was suggested, either in the way of amendment or addition, by a perusal of the British Pharmacopœias, the French Codex, and some other works of authority, was without scruple appropriated in preparing the present revision, when not too much in opposition to the modes

of practice prevalent among us, or likely to overburthen our comparatively infant pharmacy. In a work of this kind, intended solely for practical purposes, originality should never be sought after at the expense of utility; and it would be the height of folly to reject the riches accessible to us, because they were not dug out of our own mines."

There is much good sense in these observations, and the performance of the convention has not disappointed our expectations.

It is not to be looked for that the selection among various recipes for the preparation of the same medicine will always fall upon one that shall give universal satisfaction. The conditions which ought to be fulfilled in the choice are, that the formula be chemically correct; that it be concise and perspicuous; the active principle unincumbered with useless adjuncts; and, other things being equal, that it be the one most generally in use.

In examining the list of materia medica, the first change that caught our attention was in the definitions attached to the names in the second column. For example gum arabic, ammonia, assafetida, benzoin, guaiacum, manna, opium, burgundy pitch, &c. are each called "*succus concretus.*" They undoubtedly are such, and leaves, and roots, and barks, are *parts of plants;* which latter designation appears to us as appropriate in its place as the former. The old pharmacopœia defined these substances with more precision and with great propriety in most cases. Gum, the gum resins, resin, guaiacum and the balsams are well defined proximate principles in the vegetable kingdom; they convey by a single word a clear idea of the peculiar properties of the substance and are fully entitled, we think, to a place in the definitions of the materia medica.

There is a looseness in this nomenclature to which not even the authority of the London College can reconcile us, and which must, we think, strike the most careless reader. Thus camphor, castor, spermaceti and musk are defined to be *concretum sui generis;* while on the other hand a strict definition is given of many substances, pointing out their peculiar origin and the manner of obtaining them. The convention appears to us to have deviated in these cases from its own principles, which would dictate, that where the concrete is a

peculiar proximate principle, as camphor, &c. its chemical name should be designated, and where it is marked by some peculiarity in its origin, as castor, musk and spermaceti: this should be concisely defined, in the same manner as the oil of cloves is said to be the volatile oil of the flower buds of the clove tree, and neatsfoot oil the oil prepared from the bones of the bos domesticus.

We observed that lupuline is said to be *strobilorum pollen*, and although we presume the convention would say that pollen in its classical acceptation means fine flour or dust, we cannot admit this to be a legitimate use of the word. The term pollen, when used in speaking of plants, is strictly technical and should be restricted to the fructifying dust of the anthers. Neither is the lupuline to be regarded as a powder. It is a resinous exudation from the base (probably the nectaries) of the scales of the strobile and only becomes a powder by being dried. Were we disposed to be over critical we should object to defining galls to be the *nest* of the cynips. The word does not convey the proper idea, although it would not be easy to substitute one in its place.

The convention has bestowed great care and labour upon the botanical nomenclature of the materia medica, and this part of the work may be advantageously compared with any foreign pharmacopœia we have seen. The best modern authorities have been consulted, and the drugs, the origin of which was not designated in the old edition, are here referred to the plants from which they are produced. Thus colombo is now known to be the root of the *cocculus palmatus*, and myrrh an exudation from the *balsamodendron myrrha*. The names of various genera and species are also rectified in the present edition, and the authorities on which the reference is made are given in all cases. The red and pale cinchonas are the only medicines the species of which are not designated; and in this case it is making real progress to clear away the errors of preceding writers. It is now well ascertained that Mutis was not acquainted with the cinchonas of Peru, and that the species which have been supposed on his authority to yield the best pale and red barks afford only the *inferior* barks of Carthagena. We have also to commend the introduction of many new articles of the materia medica, among which we may

enumerate Pyroligneous Acid, Iodine, Rhatany Root, Lupuline, Hemlock Pitch, the Rosa Gallica, the Medulla of the Sassafras, Acetate of Soda, and Taraxacum. The reader, who will compare this department of the Pharmacopœia of 1820 with the same in the present work, must be struck with the minute attention which has been bestowed upon the revision; for there is not a page of the Materia Medica but has been greatly improved. Both the English and the Latin descriptions (with a few exceptions) are more precise, the references to authorities are more copious, and whatever may be the progress of science, the future revisers of the Pharmacopœia will find that the late Convention has removed the principal difficulties of their task, and that they will have little to do but incorporate into their work the discoveries of the day.

It is under the head of Preparations that the greatest improvements and changes have been made. We refer to this part of the work with a feeling of pride, as conclusive evidence of the great progress of the science of pharmacy during the last ten years in this country. To enumerate all the improvements of the Convention in this department would swell this article to an unreasonable length, and we must therefore content ourselves with a brief sketch of the changes introduced.

Among the omitted articles we enumerate purified vinegar, the black drop, the soda muriate of gold, all the preparations of antimony except tartar emetic, the precipitated sulphate, and the wine; the whole section of collyria, the amalgam of tin, &c. The sections of pills, infusions, and mixtures have also been much shortened. There is scarcely a formula in the work of which the language in which it is expressed, the proportions of the ingredients, or the process for the preparation, have not been revised and improved. In place of the black drop a dilute acetated tincture of opium of a constant strength is introduced; "purified vinegar" is supplied by "diluted acetic acid;" and the strong acetic acid is added to the preparations.

The formula of Vauquelin has been adopted for the preparation of hydrocyanic acid, and it is perhaps the best which

is in use. The physician who undertakes to prepare it by this mode should be aware that a considerable quantity of hydro-sulphuric acid gas is required for the complete decomposition of the cyanide, and should not venture to use the acid without ascertaining that there is none of the salt remaining undecomposed.

The formula for spirit of nitric ether, which has been adopted by the Convention, has been in general use for many years, by practical chemists, and is a great improvement in point of safety and economy, upon the old formula.

The hydro-sulphate of ammonia is no longer the disgrace of our Pharmacopœia; the gas is now directed to be made by the action of dilute sulphuric acid on sulphate of iron.

The Convention has adopted the Dublin formula for the preparation of tartar emetic, which Henry of Paris, a competent judge, has pronounced, after careful examination, to be the best extant.

It has also made a judicious alteration in the antimonial wine, by adopting the proportions of the London and Edinburgh colleges, viz. two grains to the ounce, and using the best wine in place of wine and water.

An improvement has been made in the manner of dissolving essential oils in water. They are first triturated with magnesia, and by this means a saturated solution is obtained with great quickness and certainty.

Among the most important additions to the work are morphia, its acetate and sulphate. The directions for these preparations are clear, full, and protracted; the process is simple and may be performed with complete success upon a few ounces of opium.

We are glad to perceive that the old and clumsy formula for the preparation of the sub-acetate of lead is changed for one founded upon just principles.

The iodide of potassium is introduced, and a good formula for its preparation is given. As this requires hydro-sulphuric acid gas to be passed through the solution, we are inclined to prefer the formula which directs a hydriodate of iron to be made, and then decomposed by the carbonate of potassa. We thus get rid of the necessity of preparing an offensive gas, and perform the operation with equal facility.

Sulphate of quinia is another remedy of prime importance, which has been introduced within the last ten years. The rule laid down for its preparation is that in use by our large manufacturers, and is sufficiently explicit for all practical purposes.

Under the head of syrups we notice several important alterations, viz.; the introduction of syrup of gum arabic and the improved recipes for preparing the syrups of garlic ginger, sarsaparilla, and the aromatic syrup of rhubarb. The improvements in the last two, more especially in the syrup of sarsaparilla, will be appreciated by every skilful apothecary.

The tincture of opium is made to correspond with the formula of the London College, the proportion of opium being changed from an ounce to an ounce and a quarter in the pint.

Neatsfoot oil is directed to be used in the preparation of the ointment of nitrate of mercury, it being found to form an ointment which does not become hard and brittle with age.

Among the many minor improvements of the present Pharmacopœia, it may be noticed that that of 1820 simply directed the substances which were to be made into pills to be beaten together until they formed a mass, whereas, in the work before us, the number of pills which are to be made is given in every instance. The old Pharmacopœia was drawn up with very little regard to symmetry. The quantity of a decoction or infusion, ordered to be made at once, varied without any apparent rule; of some, half a pint, and of others a pint, a pint and a half, or a quart. Ointment of stramonium was ordered to be made in quantities of fourteen pounds at a time, while a single drachm of acetate of zinc was the product of each operation for preparing that salt. In the present work these faults are very properly corrected. A pint of a decoction or infusion is in almost every case directed to be prepared, the quantity of stramonium ointment is reduced to three and a half pounds, and that of acetate of zinc increased to six ounces.

We congratulate the medical profession throughout our country upon the appearance of this excellent and judicious publication, which will raise our reputation abroad, and give an additional impulse to that spirit of pharmaceutical research

which has been awakened, in degree at least, by the influence of the Philadelphia College of Pharmacy. To institute a comparison between it and the Pharmacopœia recently prepared in New York, is altogether unnecessary. If they are to stand or fall by their merits, the *publishers* will soon ascertain their respective value, and we trust that the compilers of the latter work will have the candour to acknowledge the great superiority, in all points, of the Washington Pharmacopœia. We regret sincerely the difficulties that have arisen, and admitting that there were informalities in the meeting and organization of both conventions, we trust that the superior accuracy and scientific elegance of the work we are reviewing, will unite the suffrages of the American physicians in its favour, and accord to it that authority to which it is so well entitled.

QUARTERLY SUMMARY

OF

MEDICAL AND SURGICAL INTELLIGENCE.

I. PHYSIOLOGY.

1. *On Pulmonary Absorption and Exhalation.*—M. Collard de Martigny has lately made a series of experiments, with a view to ascertain more fully and exactly some of the points which had not been settled in relation to the respiratory function, and particularly to determine what becomes of the oxygen absorbed in respiration, and what part is taken in it by the azote.

The author establishes that a certain part of the oxygen taken in by respiration is consumed, and that a quantity of carbonic acid is formed. But what becomes of this oxygen? He does not admit that the generally received theory is true, but says that the oxygen is absorbed in the lung, and acts on the blood while in that organ, as is proved by the immediate changes observed to take place in that fluid, which passes out from the lung endowed with all the properties and characters of arterial blood,—the change, therefore, must be effected in the lung itself.

But there is a discharge of carbonic acid, at the same time that there is a disappearance of oxygen; does this carbonic acid owe its existence to the oxygen that disappeared? M. Collard shows that there is not an exact proportion between the oxygen lost and the carbonic acid discharged, and concludes that the exhalation of the latter is independent of the absorption of the former; and that the acid is brought to the lungs by the venous blood. These facts result from the experiments of M.M. Nysten, Coutanceau, and Edwards, some of which have been repeated by M. Collard de Martigny.

M. Edwards caused frogs, kittens, &c. to breathe in pure hydrogen, and although the vessels containing the hydrogen did not contain any oxygen, the respiration of the animals produced such a quantity of carbonic acid as to remove all doubts in relation to its independence of the oxygen respired.

M. Martigny substituted azote for the hydrogen, and came to the following conclusions:—

The first portion of air examined (after the experiment) always yields rather more carbonic acid than the rest.

The two or three succeeding portions produce nearly equal quantities.

The last, and sometimes the last but one, contains less; which is easily accounted for by adverting to the stupor of the animal and the feebleness of the respiration.

Lastly. The sum total of carbonic acid produced is smaller than it is stated to be by M. Edwards.

The editors of this Journal beg leave to remark that only a small part of the air contained in the lungs is changed at every act of respiration—that a much

larger quantity is contained constantly in the lungs than is taken in and breathed out again by one single respiratory act. Suppose the entire amount of air in the lungs be expressed by 200, and the quantity thrown out by each expiration to be 30, it will follow that at least 170 will be always contained in the organ. Now is it not possible that the oxygen of the 170 may give rise to the production of the carbonic acid noticed by M. Martigny. It should be observed that the first portions expired contained more and the last less of the acid, which may be referred rather to the consumption of the residual air of the lungs being more nearly completed in the last than in the first stages of the experiments. M. Martigny regards these experiments, however, as showing conclusively that the exhalation of carbonic acid continues long after the inspiration of oxygen ceases.

A tube, thirty-five or thirty-six inches high, open at one end and slightly curved, was filled with mercury, and a barometric vacuum formed. To the open end of the tube was attached the crural artery of a dog, previously dissected and cut off. The artery was kept closed by a pair of forceps. By forcing the blood along with his finger, it passed into the tube and rose above the mercury. After leaving the apparatus in situ for an hour, it was removed and its contents received under mercury, a small quantity of blood passing into the receiver along with the gas. The gas was entirely absorbed by potassa. He was surprised at the smallness of its quantity, considering the extent of the vacuum produced in the tube—which shows that the vacuum was produced by the vaporization of the serum.

The author repeated this experiment several times, and deduces from it the opinion that the blood does not contain free oxygen, but that that substance is really united and identified with the blood in the lungs, and that there can be no ground for supposing that the carbonic acid contained in the azote in which animals had breathed, is the product of preceding inspirations of oxygen; but, on the contrary, that the carbonic acid exhalation is a fact independent of absorption of oxygen in the lungs.

M. Martigny thinks that it happens in the lungs as in the alimentary canal, on the mucous surface of which are effected various absorptions and exhalations at the same time.

M. M. compared, by means of the barometer tube already mentioned, the quantity of carbonic acid in venous, with that in arterial blood. From four careful experiments he concludes, that the quantity of carbonic acid in venous is to that in arterial blood (in rabbits) as four to two. This difference goes to show that the venous blood must have parted with its gas in the lungs—therefore, if the respiration should in any case be interrupted, we ought to find the arterial blood containing a larger proportion of the acid.

He opened the trachea of a rabbit, and by means of a tube sucked or exhausted all the air he could get out of the bronchia. He then tied the trachea so as to prevent any more air from entering the lungs, and after a few minutes subjected the venous and arterial blood each to the test. The result of the trial showed that they contained nearly an equal quantity of carbonic acid.

From his experiments the following summary is deduced:—

1. That respired oxygen combines with the blood in the lungs.
2. That there is not an exact proportion between the oxygen inspired and the carbonic acid exhaled.
3. That the exhalation of carbonic acid continues long after the absorption of oxygen has ceased to take place.
4. That venous blood contains free carbonic acid; and that arterial blood contains about one-half less.
5. That in traversing the lungs the blood loses a part of its acid.
6. That if an obstacle be placed in the way of the expiration this loss does not take place, but its quantity does not sensibly increase in the venous, while it is nearly doubled in the arterial blood.

M. Collard de Martigny therefore supposes, that the absorption of oxygen in the lungs is a part of the function of general nutrition, and the exhalation of carbonic acid is merely the excretion of a gas secreted by the lungs.

An opinion has been entertained, and defended recently by M. Despretz, that a portion of the oxygen absorbed unites with the hydrogen of the venous blood

to form water. M. Martigny shows, that if hydrogen and azote should meet in the lungs under circumstances favourable to the oxydation of the hydrogen, a part of this gas would also pass into the state of ammonia, by combining with the atmospheric air; that every thing that can be alleged in favour of the formation of water is equally applicable to the composition of ammonia; there is the contact of the elementary gases, a nascent state of one of them, temperature, electricity, the analogy drawn from inorganic chemistry and the decomposition of vegetable and animal substances.

But, from numerous experiments performed, it is proved that not a trace of ammonia exists in the matter of the pulmonary transpiration. The production of this fluid cannot therefore be attributed to the oxydation of hydrogen. And besides the pulmonary halitus remains about the same *ceteris paribus*, whether the animal respires in atmospheric air or in azote. The quantity of the pulmonary halitus is found (by experiments) to vary according to the quantity of air inspired, the number of respirations, the temperature, &c. upon principles very similar to the laws of common evaporation; whence M. M. concludes, that the halitus is deposited upon the bronchial surfaces by exhalation and then vaporised and excreted.

By means of experiments in which he isolated the azote exhaled or absorbed by the skin, the digestive tube, and the lungs, M. M. found that the quantity of the azote exhaled is very variable, and seems connected with the proportion of carbonic acid.

We shall not extend this article any further than to exhibit M. Martigny's general statement of the results obtained, or supposed to be obtained by him.

1. There takes place in the lungs both *absorption* of oxygen and *excretion* of carbonic acid, azote and serosity; *imbibition* of oxygen and azote, and *exhalation* of azote and carbonic acid.

2. The two first named acts constitute the function of respiration; the others are common to all the organic surfaces.

3. The oxygen absorbed in the lungs combines there with the blood, and does not circulate as free oxygen.

4. The carbonic acid expired is a product of the *assimilated decomposition, secreted* in the capillaries and *excreted* by the lungs.

5. The azote expired appears to be a secretion from the blood itself, effected instantaneously in the lungs.

6. The respiratory serosity is deposited on the pulmonary surface by *organic exhalation*, and dissipated by *evaporation*.

7. Lavoisier's chemical theory of respiration is a gratuitous hypothesis.

8. This function ought to be regarded as a complex series of acts of the general assimilation.—*Archives for October*, 1830.

2. *Animal Electricity.*—At the sitting of the Royal Academy of Sciences at Paris, 25th of October, Dr Person read a memoir on animal electricity, and on a new instrument intended to show instantaneous electric currents. Among the conclusions to which the author has been led are the following:—1. It is not necessary for the electric current to pass along the whole nerve to produce muscular contractions; the shock takes place no matter how small the extent to which the nerve has been traversed. 2. The existence of electrical currents in the nerves during life is a gratuitous supposition. 3. The only well ascertained case of animal electricity is contrary to this hypothesis. 4. The commotion excited by fishes, called electrical, is really due to electricity.—*Archives, Nov.*

II. PATHOLOGY.

3. *Endemic Parotiditis.*—In the concluding number of the *Journal Universal*, we meet with an account of parotiditis which occurred endemically among the soldiers of the first regiment of light infantry during their stay at Mont-Louis,

Eastern Pyrenees, in 1828, from the observations of Dr Dongy, surgeon-major of the regiment. The disease was first observed among the recruits who arrived in the month of November 1827. In the beginning of January following, of the eight hundred men who constituted the garrison of Mont-Louis, eighty-three had become affected. The disease was one to which the inhabitants of the place were unaccustomed, and Dr Gambus, the only physician of the city, had never met with a case of it during a practice of thirteen years. Notwithstanding the number of patients who were attacked with this complaint very few died. Metastases were frequent, but the affection of the parotids was in no instance complicated with exanthemata or petechia. The medium duration of the disease was from eight to twelve days. Nothing remarkable was observed in regard to its termination, if we except its frequent metastasis to the testicles. In a single case, in which it was complicated with otitis, it pursued a chronic course, notwithstanding the employment of warm drinks, antiphlogistics, &c. The period of incubation was marked by general uneasiness, lassitude, pains in the limbs and loss of appetite. Fever was seldom present. The duration of this period was from two to four days: a swelling now occurred in the region of one or both of the parotids, and extended itself frequently to the whole of the anterior part of the neck. In sixty-nine cases the disease affected both parotids, and in eighteen only one of them. As already mentioned, metastasis to the testicles was frequent; in four instances inflammation occurred in both testicles subsequently to an affection of both parotids; and in twenty-three cases but one of the testicles became the seat of inflammation. The testicles thus affected were all atrophied, as was ascertained by an examination made several months after the removal of the disease. In a solitary case the testicle of the opposite side from that on which the parotiditis was seated became inflamed. Dr Dongy never saw a case of metastasis from the testicle back again to the parotid gland, nor did a sudden cessation of the inflammation of the latter ever give place to any other affection than that of the testicle. Dr D. has also assured himself that two only of the patients had previously been affected with inflammation of the parotids. In no instance did suppuration occur either in the neck or in the testicles. The individuals in which the disease occurred were from twenty-two to twenty-eight years of age; one only beyond the latter age was affected, this was a soldier of thirty-two years of age. Of those attacked with the disease the greater part were recruits who had arrived at the garrison about six weeks before it made its appearance. Dr D. is convinced that the exciting cause of the inflammation was fatigue conjoined with exposure to cold and dampness. The soldiers of the garrison were exercised during the months of December, January, and February in galleries exposed to currents of wind, and in which there was a considerable quantity of snow; after exercising they returned immediately to their apartments, where they were crowded together in a temperature frequently of very considerable elevation: from these they were again called forth, either to recommence their exercises, or to form roads through enormous masses of snow. The place of birth Dr D. believes to have had some influence in predisposing to the attack of parotiditis: thus of those who were affected with it thirty-six were from the upper Pyrenees, twenty-four from the lower Alps, eighteen from Corsica, six from Var, two from Ardennes, and one from Calvados. All were of a sanguine temperament and of a fine constitution. Almost all were from warm, dry and mountainous situations, which they exchanged for one of the most elevated points of the Eastern Pyrenees, where the cold reigns during the whole year, and where, during eight months of the twelve, heavy snows are the cause of a very great degree of humidity. The disease, also, was most common in those soldiers who slept near the doors or windows, and especially in such as were intemperate in the use of wine. The latter constituted two-thirds of the entire number affected.

4. *Ischuria Renalis.*—The following case is copied from the fifth volume of the Transactions of the Medical and Physical Society of Calcutta, in the January number (1831) of Johnson's Review. Ischuria Renalis is a very unfrequent but very dangerous disease. Many medical men, of extensive practice, have never once

seen it. When met with, it is not always that a post mortem examination is obtained.

The patient in the present case was an invalid of the garrison of Tannah, in the East Indies, aged fifty years, who was admitted into the regimental hospital on the 18th of October 1818, complaining of paucity of urine; not more than a small tea-cupful in the twenty-four hours being voided. He had nausea, but no fixed pain in any part. When first attacked, several hours before admission, he experienced some uneasiness in the lumbar region, attended with vertigo. He was ordered a dose of castor oil, and afterwards a draught with tincture of opium and nitrous ether. On the second day there was no alteration in the symptoms. The draught was several times repeated. On the third day the sensorium appeared much affected, as marked by drowsiness, unsettled state of the mind, and loss of memory. His eyes were suffused, yellow, and slightly injected; pulse slow and full; some convulsive motions of one arm. The catheter was introduced into the bladder, but no urine was found there. He was bled to twenty ounces, took four grains of calomel, with the same quantity of antimonial powder, and then had a blister to the lumbar region. The blood was cupped, and slightly buffed. *Fourth day.* Less affection of the sensorium; no increase of urine; a brisk purgative was directed, with a blister to the nape of the neck. *Fifth day.* Sensorial disturbance increased; great drowsiness; tongue brown and hard; purgative medicines produced no effect. *Sixth day.* Coma continues to increase; bowels obstinately confined. The patient died on the next day.

Dissection.—The stomach was much thickened, and its villous coat very vascular. The liver was enlarged; it displaced the intestines, and had elongated the mesentery. The spleen was enlarged to five times its natural size(?), and was very firm. The colon was contracted throughout its whole extent. The kidneys were enlarged to double their usual size, and were much altered in structure, having the pelves unnaturally small, and the mamillary processes so changed as to be with difficulty recognised from the general mass of disease. The impression on Mr Bird's mind (the gentleman by whom the case is reported) was, that the urinary secretion must have been imperfect for a length of time. In the head, there was a general serous effusion throughout the substance of the brain, and into the lateral ventricles. No mention is made of effusion having taken place in any of the other cavities or into the cellular tissue. Mr Bird is disposed, but we cannot see upon what foundation, to connect ischuria renalis with liver affection rather than with organic disease of the kidneys.

5. *Hæmatopedesis.*—A case of this curious affection is related in a memoir of M. Chauffard of Avignon; read before the Medical Society of Paris in October 1830.

A girl of twenty-one years of age, affected with irregularity of the menses, having a brain but slightly developed, and feeble intellect—of an indolent and obstinate disposition, was chagrined by the reproaches of her parents, in consequence of her having renounced the protestant religion. She left her home, and sought an asylum with various persons; finally she was admitted into the hospital. She was at the time of her admission subject to regular attacks of hysteria, marked by general convulsions, extreme sensibility of the pubic and hypogastric regions, a sense of strangulation, hiccough, sighs, &c.

When the attack of hysteria was violent and continued from twenty-four to thirty-six hours, the patient passed into a kind of extacy—the eyes becoming fixed and without any appearance of intelligence, and the movements of the body defective and automatic: the patient occasionally muttered prayers, and an exudation of blood took place on the cheeks and the epigastrium. The whole of the sub-cutaneous capillary tissue in the parts at which the hemorrhage took place was injected, the skin being of a bright red and covered with vascular arborescences. The hemorrhage recurred whenever the paroxysms of hysteric catalepsy continued for any length of time. The patient continued in the state here described nearly three months; her disease was treated at first, without success, by local depletion from the head and the genital organs; it ceded finally

and "with great rapidity to repeated revulsive bleedings and to other revulsive means."

6. *Effects of Posture on the Pulse in Health and Disease.*—The fifth volume of the Dublin Hospital Reports contains a very interesting paper by Dr Graves "On the Varieties produced by Posture in the frequency and character of the Pulse in Health and Disease." For the following analysis of the author's observations we are indebted to the Ed. Med. and Surg. Journ. for Jan. 1831.

It is known that considerable variety, both in strength and frequency of pulse, is observed in the erect, the sedentary, and recumbent posture. Dr Thompson of Edinburgh first pointed out some of these differences from an extensive series of accurate observations; and these were confirmed by Dr Stroud in the numerous experiments made by him on the patients of the clinical wards in the years 1816 and 1817. Unaware of these experiments apparently, Dr Graves has, with extreme diligence, devoted himself to the observation of the varieties of the pulse, not only in the erect and recumbent posture, but in the inverted, with the feet above and the head dependant. The author found, however, that this position produced much less effect than he anticipated. The frequency of the pulse in this position was neither retarded nor accelerated; but its strength was diminished, sometimes very considerably, and occasionally it became very irregular; a circumstance which Dr G. is inclined to ascribe to the weight of the column of blood pressing on the aortic valves, and thus augmenting the impediment to its exit from the ventricle.

Dr Graves found the pulse less frequent, from six to fifteen beats in the minute; and stronger in the horizontal than in the erect posture; and he infers, that in this position its maximum of strength and minimum of frequency are at once attained. This fact he thinks may explain the relief obtained by placing patients in the horizontal position in order to avoid fainting, for instance, that produced by venesection. This however, with deference to the author, we must be permitted to question. Syncope from venesection is doubtless occasioned by the blood being so suddenly drawn off that there is no longer a sufficient quantity left to excite the action of the heart; and it is with equal certainty obviated by placing the patient in that position, in which, while the blood is drained from the inferior vessels, it may, by being gradually allowed to flow from the superior ones, still be adequate to excite the heart.

In all diseases, except six cases of hypertrophy with dilatation of the heart, Dr Graves found the pulse to differ in frequency in the erect, sitting and horizontal postures. In four of these six cases, the fact of hypertrophy with dilatation was ascertained by inspection; and of the other two, in one it was certain during life, and in the other was at least probable. It would be doubtless premature to speculate on the cause of this peculiarity until the phenomenon has been observed in a greater number of cases. We conclude our account of this paper by subjoining the general conclusions, which Dr G. thinks may be established from an extensive number of observations.

"1. That the greatest difference occurs in patients labouring under fever, or in a debilitated state, in consequence of fever or any other cause. It may amount to thirty, forty, or even fifty, between the horizontal and erect postures.

"2. That this difference decreases after the first quarter of an hour, in most cases, but always remains considerable as long as the same position is observed.

"3. That in persons not much debilitated, the difference is much less than that stated above, and often does not amount to more than ten.

"4. That, when the patient lies down, the pulse rapidly falls to its former standard.

"5. That in some the frequency is greater between the horizontal and sitting posture, than between the latter and the erect; while in others the contrary takes place, so that generally the frequency in the sitting posture may be taken as a mean.

"In persons convalescent from fever or acute diseases, I find it is extremely useful to the physician to ascertain the comparative frequency of the pulse in the

horizontal and erect position. The greater the difference the greater is the debility of the patient; and, consequently, the more guarded must his medical attendant be in allowing him to sit for any length of time, particularly if the pulse, on his lying down, does not resume its usual degree of frequency."

7. *Cancer and Ulceration of the Colon.*—For the following very interesting case we are indebted to the Medico-Chirurgical Review for January 1831.

A man aged thirty-one years, of delicate constitution, had enjoyed good health until within a year of his admission into the hospital (the Hotel Dieu, at Paris). He then began to experience various painful sensations in different parts of the abdomen, but especially about the hollow of the left ilium. His appetite was not impaired, but his digestion was deranged, and he had frequent attacks of colic. The discharges from the bowels were in general liquid, consisting of bile or mucus,—sometimes containing clots of blood. He soon began to lose both strength and flesh, when a tumour appeared on the left flank, which was both visible and tangible. It was not until after eight months of suffering that this man applied at the Hotel Dieu for relief. He was then in a state of extreme emaciation and debility—had lost his appetite—and was much harrassed with diarrhœa. The supra-iliac tumour was hard and about the size of a man's fist. It was very tender to the touch. It was pronounced to be cancerous,—and little doubt was entertained that it was in a state of ulceration. Nevertheless, it was determined to employ pressure on the plan of Mr Young, of England, and some ingenious instruments were contrived for effecting this measure. In six or eight days, it was ascertained that the volume of the tumour was considerably diminished without any augmentation of the patient's sufferings. The pressure was continued for fifteen days, when the tumour ceased to diminish, and became harder as well as more tender. The diarrhœa and emaciation now made rapid progress, fever came on, the evacuations became extremely fetid, bloody and puriform. Death soon put an end to the patient's sufferings.

On dissection the stomach was found to be very large, softened, and reddened on its internal surface. The mucous membrane of the small and great intestines was developed in a most extraordinary manner, and of vast thickness. As the lower portion of the colon was approached, this thickening became more remarkable, and changed, in fact, into a sub-schirrous condition. The sigmoid flexure of the colon was the seat of the tumour, which, indeed, was formed by the thickening of the intestine itself. Externally, it was very dense in structure; but internally it was soft, and assumed the fungoid or encephaloid character. In its centre was a large cavity with irregular parietes, which had furnished the fetid and purulent discharges during life.

It was but very recently, adds the editor of the Review, that we met with a true fungoid or malignant cancer of the pylorus, in a female about fifty years of age. She had experienced daily sickness after food for more than a year; but the complaint was greatly exasperated by long continued sea-sickness in the steamer between Ostend and London. From that time till the day of her death, six or seven weeks afterwards, she was not able to keep any thing on her stomach. Yet she preserved her corpulency till the last. About three weeks before death we examined carefully the abdomen, and perceived a slight hardness in the region of the pylorus, but it was unattended by tenderness. She was seen by Dr Granville and Mr Guthrie in consultation, but no remedial measure could stop the incessant vomiting after even the most trifling quantities of fluid, taken into the stomach. The argentum nitratum allayed the irritability of the stomach more than any other remedy, but it gave only temporary relief. The patient was worn out by constant vomiting, fever and inanition. On dissection the parietes of the stomach, in the neighbourhood of the pyloric orifice, were thickened and indurated,—but from the mucous surface of this thickened part a malignant fungoid tumour projected, and blocked up the passage into the duodenum, which would with difficulty admit the point of the little finger. This fungoid prominence was in a state of ulceration. It is not a little remarkable, that the fat was an inch and a half in thickness on the abdomen, notwith-

standing this long continued sickness occasioned by an organic disease of the stomach.

8. *Dropsy of the Liver.*—This case is related by Dr Hesse in *Horn's Archiv. &c.* for September and October 1829. We derive the present notice of it from the Archives Générales, for October 1830. An unmarried female, by trade a seamstress, who had always enjoyed good health until within a few years, when in consequence of a fall she struck the right side of the abdomen, over the region of the liver, upon a pointed stick. Various remedies were immediately made use of, both externally and internally; the abdomen, however, gradually augmented in size, and finally the chest became affected. The patient applied to a physician, who pronounced her to be labouring under ascites, consequent upon neglected hepatitis. He directed various remedies from which no relief was obtained; parecentesis was then performed on the right side of the abdomen, and repeated on the same side, but higher up,—no discharge of fluid took place. Dr Hesse was now called in. On examining the patient he found the abdomen enormously swollen, and projecting towards the umbilicus in form of a cone, which was surmounted by a tumour about the size of an infant's head. The umbilical ring was free and dilated. The tumour was light, movable, and appeared to communicate internally with the cavity of the abdomen. The patient experienced great difficulty in respiring in a horizontal position. There was a constant inclination to evacuate the bowels, but nothing was discharged. There were no traces of œdema, but every portion of the body, with the exception of the abdomen, appeared, as it were, momified. The pulse was scarcely perceptible. The patient was all the time lively and inclined to gaiety. Dr H. immediately passed a trochar through the umbilical ring, and gave issue to twenty-seven pints of an inodorous, mucilaginous, and somewhat turbid fluid. Portions of a fibrinous substance, which adhered firmly within the abdomen, insinuating themselves into the canula, put a stop to a further escape of the fluid. A catheter was now introduced into the canula; it penetrated to the depth of nine inches without coming in contact with the sides of the cavity. For eight days nearly five pints of serum were discharged daily by the opening, from which the patient appeared to experience relief,—and under a restorative treatment, in the space of a few weeks, she was so far improved in health, as again to return to her usual employment. The abdomen still, however, remained tumid and hard, and the digestion difficult. At the end of a year the patient was again obliged to keep her bed; she died in a few months; during this period no fluctuation could be perceived within the abdomen.

Dissection.—On opening the abdomen, the diaphragm was found to be thrust up on the right side as high as the second rib, and on the left, to the fourth. The heart and lungs were very small. There was no trace of omentum, nor of mesenteric glands. The pancreas was destroyed (?). The stomach, intestines, spleen, kidneys, uterus, and bladder, were of an extremely small size. The liver, which was entirely free over the whole of its entire surface, filled completely, as it were, the enormously large cavity of the abdomen. The fluid which had been evacuated by puncture through the umbilicus, appeared to have been contained entirely in the substance of the liver. This organ was of a deeper colour than natural,—its left lobe was greatly enlarged, extending to the fourth rib of the left side,—its parenchema was very loose, and its vessels dilated. The lobus spigelius presented nothing unusual. There existed no gall bladder, its place being occupied by bands of a cellular structure, small ulcers, and some biliary calculi about the bigness of nuts, each one of which was surrounded by a kind of membrane. The biliary duct was obstructed, although during life the patient had presented no icteric symptoms; her skin being rather of an ash colour. The inferior surface of the liver was covered with cellular excrescences, and with superficial ulcers. The superior convex surface adhered intimately with the diaphragm. Within the right lobe, which was enormously enlarged, a fluctuation was perceptible, and an incision into it gave issue to twelve pints of sero-

sity, at first of a watery appearance, then turbid, and flocculent; followed, finally, by a kind of sanies without smell, and which covered the whole of the internal surface of the cavity formed within the right lobe. The parietes of this cavity were only half an inch in thickness. From the circumstance of the serosity being discharged during life by the opening made through the umbilical ring, it was evident that the umbilical vein had been rendered permeable, and that through it the cavity in the liver had communicated with the umbilicus. Besides this, no other communication existed between the liver and the peritoneum. It is worthy of remark, that notwithstanding this extensive lesion of the liver, the patient's gaiety of temper was never impaired. It is probable, though Dr H. does not say so, that in this case the dropsy of the liver was connected with development of hydatids within its substance.

9. *Instance of Universal Hypertrophy.*—A female of a robust constitution, five feet two inches in height, menstruated regularly and abundantly until about her eighteenth year, when in consequence of a violent storm by which she was greatly frightened and wet to the skin, the menstrual flux, which had existed for some days, was suddenly stopped and did not again appear, nor has she had the least trace of it up to the present period, eleven years from the time of its suspension. The inconveniences she experienced in consequence were a shortness of breath, heaviness of the head, and a numbness of the limbs: to these symptoms she paid very little attention. She continued to follow her rustic employments, and felt perfectly happy at the circumstance of her freedom from her monthly evacuations. Soon, however, the muscles of this female, the cellular and adipose membranes, and even the skin began rapidly to increase in bulk and attained finally a very remarkable development. Her appearance, when seen by Dr Claubry, by whom the case is related in the *Journal Hebdomadaire*, Vol. I. p. 76, was as follows. Height five feet two inches as formerly; cranium of the size and proportions ordinary in a female of her stature, but appearing very small in comparison with the size of her face, which was enormously developed. The skin on the forehead, eyebrows and lids, nose, lips and cheeks presented a thickness, projection and volume such as we might conceive would be adapted to the mask of an individual seven or eight feet in height. The soft parts of the face were prominent and at the same time pendulous—the chin descending before the neck much more than it should do were the feature well proportioned. Between lips of enormous thickness, circumscribing a mouth of giant size, passed out the tongue, broad, thick and long, which filled the cavity of the mouth, while the point was bent over the inferior teeth. A constant salivation gave a disgusting appearance to the aspect of the patient, and her speech was impeded by the hypertrophied condition of her tongue. Her neck was of great thickness, divided lengthwise by thick folds of the skin raised by the masses of fat beneath; this was especially the case at its back part. The mammæ were of enormous size, covering the whole front of the thorax and reaching within a little of the chin; they were coated with a thick and adhering skin. The trunk of the body appeared remarkably short in consequence of its great thickness and breadth. The abdomen was remarkably prominent, and the dorsal and lumbar regions were of uncommon breadth. Notwithstanding the thickness of the fat and skin by which the muscles were covered, those of the trunk were readily recognized, especially when the hand was applied upon the back, loins or abdomen, when the muscles were thrown into action. The labia pudendæ, &c. were very much augmented in bulk. The limbs, both superior and inferior, appeared, at first view, disproportionably short, in consequence of their immense bulk. The layer of fat was less abundant on them than on the body, and the muscles appeared developed to a remarkable extent. The hands presented a singular appearance—the bones, as in every other part of the skeleton, retaining their normal size, while the soft parts were excessively developed. When the hand was opened, its great thickness with its immense fingers, which in consequence of their bulk appeared uncommonly short, caused it to resemble nothing but a very large sized racket for playing

ball. The feet presented a similar disproportion between their length and thickness. The heart of this female appeared likewise to have experienced, in common with the rest of the muscular system, an unusual development; it occupied a very large space behind the sternum and in the left side of the chest. Its pulsations were very strong, and communicated a shock to the head of the examiner when using the stethescope; but no abnormal sound could be detected during the passage of the blood through its several openings, while the perfect correspondence between its movements and those communicated to the arterial tree removed any doubt as to its healthy condition. The encephalon would appear not to have been exempt from the same disposition to hypertrophy displayed by the other soft parts, though the solid parietes by which it is enclosed prevented an actual enlargement of its bulk. The patient, however, who until her eighteenth year was far from being without intelligence, subsequently became gradually obtuse in her intellectual powers, and finally fell into a state of partial idiotism. She complained, also, of ringing in the ears, vertigo, heaviness of the head, &c. phenomena, all of which may be referred to the increased amount of blood driven into the brain by the hypertrophied heart, and to the resistance offered to the enlargement of the cerebral mass by the cranium. The patient from the same cause was constantly disposed to sleep and to a general state of numbness, to get rid of which she engaged in the most active bodily exertions. She has still an excellent appetite, drinks very little and is habitually costive. To remedy the plethoric condition of her system, from which she suffers inconvenience, she frequently has recourse to leeching. The foregoing case is one of considerable interest; it is not merely an instance of excessive plethora from suppressed menses, nor a simple adipose polysarcy; the body of the patient presented absolutely an excessive nutrition in all the soft tissues including the muscular. Her idiotism appears incurable, more especially as the patient could not be brought to see the propriety of abstinence, the use of watery drinks, acids, &c. It is added that this herculean female presented no symptoms of the venereal passion.

10. *Puerperal Peritonitis with Perforation of the Œsophagus.*—We copy this interesting case from the *Archives Générales* for November 1830. It is related by Dr Lachéze, Adjunct Professor of the Secondary School of Medicine of Angers, and appeared originally in the *French Lancet*. Aglaé Ch., aged fifteen years, came to l'Hospice de la Maternité to lye in. During the period of her pregnancy her health was perfectly good, until within five days of her confinement, when she was attacked with violent colic and diarrhœa; she had nevertheless a favourable delivery on the 18th December 1827. The lochial discharge was very slight and no milk appeared in the breasts. The abdomen remained tumid and the disease of the bowels persisted, notwithstanding the use of opiates, emollients and a restricted diet. This state of things continued until the 27th of the month, when there occurred violent pain of the abdomen, increased upon pressure, constant inclination to vomit, with the occasional discharge from the stomach of a dark coloured fluid, a sense of constriction in the chest, and pain about the neck increased when the parts were touched. She was directed a pectoral ptisan, an antispasmodic potion, and an absolute diet. On the 23d the symptoms were less severe, the diarrhœa had ceased; on the 29th, however, it again recurred, to a greater extent than before. Injections of starch and laudanum, ptisan of rice, an anodyne potion with syrup of quinces were directed; diet as before. The patient exhibited no alteration in her symptoms up to the 4th of January 1830. Her features were now considerably altered and hippocratic. On the 5th, pain of the abdomen renewed. Six leeches were applied to the vulva. On the 6th the symptoms assumed an alarming intensity; face very pale; at intervals the pain of the neck and sense of oppression increased greatly in intensity. The patient died in the evening, her consciousness being not the least impaired. *Autopsy* fourteen hours after death. The cranium was not opened. On the right side of the neck there existed a disorganization of the muscles and cellular tissue, which were converted into a blackish pulp, of a sweetish smell, analogous to that of the fluid which was contained in the chest and stomach. The disor-

ganization in the neck extended upwards to the angle of the jaw. The vessels and nerves had the appearance of being dissected. The same kind of disorganization existed in the posterior mediastinum, along the vertebral column. The cavity of the thorax was filled with a blackish fluid, tolerably thick, having a sweetish smell, as of syrup. The lungs were collapsed and presented slight adhesions with the pleura. The posterior mediastinum communicated, at the point between its upper third and lower two-thirds, with both cavities of the chest, into which was discharged from it the black fluid already noticed. The œsophagus was perforated to the extent of nearly two and a half inches. From the cardiac orifice of the stomach to the first bronchial ramification all that remained of the œsophagus was about two lines breadth of its posterior parietes, the edges of which presented a fringed appearance. The portion which was left offered no traces of disorganization nor alteration in its texture. The abdominal cavity was filled with a sero-purilent fluid, and its various viscera were united together by numerous points of adhesion. The upper portion of the intestinal tube was healthy, but all the large intestines and the inferior portion of the ileon were in a softened state with thickening of the mucous membrane. The uterus was contracted and perfectly healthy.

11. *Spinal Apoplexy.*—The following case of hæmatorachis, or spinal apoplexy, is related by M. Fallot, physician to the hospital at Namur, in the *Journal Complémtaire des Sciences Médicales* for October 1830. A man twenty years of age, of middle stature, muscular, with light hair, blue eyes, without beard, who had previously enjoyed good health, lost by degrees his appetite and strength, and was affected with hæmoptysis during three months. He was not, however, prevented from continuing his ordinary occupations, until the 11th of May, when he was sent to the hospital. His symptoms then were, face red and tumid, with œdema of the eyelids, light cephalalgia, extreme lassitude and pains of the limbs; intellectual faculties unaffected; pulse without frequency; skin without heat; respiration easy and equal; a slight disease of the throat, and difficulty of swallowing; the fauces without redness. He was directed pedaluvia and common ptisan. In the evening there occurred a sudden congestion of the brain, marked by a loss of sight, vertigo, injected eyes, and small and contracted pulse. Fourteen ounces of blood were taken from the arm. The pain in the head, the intumescence of the face and injection of the eyes now disappeared; he passed, however, a disturbed night. On the 12th, the head free from disease; face very red; œdema continued only in the left inferior eyelid. There was extreme inquietude; with continued agitation from the intense pains of the limbs, which were thrown occasionally into spasmodic contractions, during the continuance of which the face became of a bluish tinge, and the head forcibly thrown back, every attempt to bring it to its natural position giving rise to sharp pains. The cervical region tumid, and hot, and painful upon pressure. Skin rather cold than hot; pulse small, slow, and concentrated; sensibility of the surface reduced; tongue clean, flat and soft; no thirst; excretion of urine; bowels constipated. Forty leeches were applied to the nape of the neck with a purgative enema, and gum water acidulated for drink. During the day the symptoms diminished, but colicky pains occurred, which required the application of ten leeches along the course of the colon. On the 13th, lungs engorged, presenting a crepitant rattle throughout their whole extent; every attack of coughing caused the expectoration of a mouthful of a sero-sanguinolent fluid, covered with abundance of thick foam. The features were altered; face of a bluish colour and tumid; respiration short, rattling and hurried; pulsations of the heart rapid and confused; pulse thready; extremities cold; the spasmodic contractions have disappeared, and the muscles move in obedience to the will. The patient was able to raise himself in a sitting posture, but could not retain this situation from the sense of suffocation which it occasioned. The head may be inclined upon the chest without inconvenience, but falls backwards when left to itself. The skin has regained its natural sensibility in every part excepting over the cervico-dorsal region. The whole body exhales a nauseous and penetrating odour. Cups deeply scarified were applied to the nape

and back. These caused a considerable discharge of blood, and immediately afterwards the sensibility at this part was renewed. The respiration became less short and rattling, and the skin somewhat warmer. Twelve leeches were directed to be applied near the great occipital foramen, with hot sinapisms to the back, and blisters to the wrists. During the day the patient was tranquil, repeating several times that he felt very well and without pain. At four o'clock he turned over on his back, and immediately expired without an agony. A few moments after death his face and lips became swollen, the latter being of a violet colour. The penis was in a state of partial erection.

Autopsy seventeen hours after death. The limbs flaccid. On cutting into the muscles of the cervical region, there escaped a considerable quantity of black fluid blood; this occurred at no other part. The surface of the cerebrum and cerebellum was of an obscure red, depending in part on the extreme distension of the meningeal vessels and sinuses by dark blood, and in part upon the infiltration with a bloody serum of the sub-arachnöidien cellular tissue. The substance of the cerebrum and cerebellum presented no abnormal redness; it was firm in every part with the exception of the cerebral triangle, the anterior pillars of which were sensibly softened. A little reddish serosity existed in the lateral ventricles. Under the arachnoid membrane, at the internal edge of the lower surface of the left cerebral lobe, there was found about a coffee spoonful of black coagulated blood. The vessels of the rachidien meninges were strongly injected. On the posterior surface of the spinal marrow was found, on the level of the fifth pair of cervical nerves, beneath the arachnoid, which was raised up in consequence, a layer of coagulated blood, which, confined at first to the space of a few lines of the right cord, afterwards enlarged, and varying merely in thickness, covered the whole of the posterior surface of the spinal marrow to the origin of the cauda equinæ, where it terminated abruptly. On the anterior surface of the spinal marrow were observed numerous small distinct clots of blood of irregular form, and laying on the spinal nerves either of the right or left side. The first of these clots was immediately without the occipital foramen, and hence much higher than where the posterior layer of blood commenced. About the middle of the dorsal portion of the spinal marrow there existed a coagulum of about two inches in length which occupied the whole width of the canal. These coagula, and the layer of blood posteriorly, were firmly attached to the subjacent parietes and to the fasculi of nerves. The substance of the spinal marrow had undergone no alteration either in its consistency or colour. Beneath the effused blood, however, the membrane by which it is immediately enveloped was of a much deeper red than in other parts, and was not rendered paler by washing. The lungs were engorged with black and frothy blood, and crepitated throughout their whole extent. About a pint of a bloody serum existed in the cavity of the left pleura. The right pleura adherent. Heart of a size larger than usual. The parietes of the ventricles thickened. Venous system filled with blood. The right auricle contained an enormous coagulum; the left cavities empty. A bloody serum was contained in the peritoneal cavity. An arborescent and punctulated injection existed on the greater part of the ilium; a few oval patches of a brownish red, with small spots of a bluish black, existed upon the free surface of the termination of this intestine.

III. THERAPEUTICS.

12. *On Strychnia in the treatment of Paralysis.*—Our contemporary, the American Journal of the Medical Sciences, contains an essay on the above subject by Dr E. Geddings, lecturer on anatomy and surgery at Charleston, S. C. Dr G. details a few cases in illustration of the efficacy of the remedy in several varieties of paralysis.

The first case was one of hemiplegia of the whole of the left side of the body. The patient was a male, and aged about fifty years. He was placed under treat-

ment on the 20th of July 1829. Had been suddenly attacked almost a month previous with violent pain in the head, left arm and leg. He fell down in a state of insensibility, succeeded by a total loss of sensation and motion of the parts mentioned, and double vision. When visited, the double vision had disappeared; there was no fever; speech was unimpaired. The patient was ordered to take cathartics, and to have the head blistered. Two days after, the medicine was repeated; cups were ordered to the nape of the neck, the parts afterwards to be covered by a blister. Other stimulating external remedies were subsequently employed. This plan, however, proving ineffectual, the tincture of strychnia was resorted to on the 24th of July. On the 28th, the pain was relieved; motion and sensibility had improved; the muscles of the arm were affected with twitchings; the paralytic arm and leg were noticed to be œdematous. The improvement gradually progressed, particularly in the leg, the arm remaining, on the 4th of August, nearly the same. In about a week from this time the patient began to walk with the assistance of a crutch, and shortly after, the power of motion in the leg was so far restored as to enable him to walk to any part of the city. He finally recovered sufficiently to be sent into the country. The arm, in the meantime, had not regained its power, and had undergone but a very slight amelioration.

Dr G. thinks that in this case the paralytic symptoms originated from an extravasation of blood in, or upon some part of the brain. Hence his first indication was to subdue irritation by cups and blisters, and to create a revulsive impression from the brain to the bowels and extremities by means of cathartics, and sinapisms applied to the legs. Considering with most writers that strychnia and other remedies of the sort should not be used in cases of paralysis arising from recent attacks of apoplexy, he resorted to it in the above instance only after having employed other means without benefit, and because he considered the case of a desperate character.

In the second case, the paralysis was complete. The patient was a man twenty-eight years of age, and had been in the hospital some months. His legs and arms were so completely powerless as to render him incapable of helping himself, and whatever he took by way of food or medicine had to be put into his mouth by another person. His legs were drawn up to a right angle with the thighs, and could not be straightened. His bowels had, moreover, so far lost all power as to be incâpable of expelling their contents, except when inordinately excited by stimulating enemata. The disease was attributed to the long continued and intemperate use of bitter beer and ardent spirits, especially gin, which had probably been adulterated with acetate of lead.

In this case the strychnia was administered by the stomach, and also, together with aloes, in the form of enemata. By the latter mode it was intended to stimulate the muscular fibres of the large intestines. This plan was instituted on the 15th of January. "By the 1st of March the mobility of the members had become so far restored, that nothing seemed to prevent the patient from walking but the flexed position of the legs, and a want of confidence in his own powers. These difficulties were gradually surmounted by means of friction and exercise, and at the present time (September 1830), though he still experiences a little stiffness of the members, he can walk without assistance, and descends and ascends almost daily a long flight of stairs. The bowels have been for some time past in possession of their natural tone, and evacuate their contents without the aid of medicine. The general health of the patient is entirely restored."

Two more cases are related. They appear to have been produced by the same cause as the preceding one. In these the paralysis was limited to the fore-arms and hands. In both the same plan of treatment was highly beneficial. Dr G. remarks in conclusion, that he has employed strychnia with much apparent benefit in some forms of chronic irritation of the alimentary canal, especially in persons who were affected with habitual constipation of the bowels; but adds, however, it should always be borne in mind, that the natural tendency of the remedy is to irritate the mucous membrane.

13. *On external Medication.*—Dr W. M. Fahnestock of this city has published an essay on this subject in the last number (February 1831) of the American Journal of the Medical Sciences. This essay contains some very useful practical precepts, and does the author much credit. We cannot help thinking, however, that Dr F. does not do himself full justice by quoting so profusely the very language of innumerable authors; by stringing quotations by the dozens on the most common topics. He remarks, that it is a favourite practice with him in discussing subjects, to fortify his opinions with the facts and observations of established authorities, and that, consequently, on the present occasion, he will not consume time with a detail of his own experience. As regards ourselves, we believe, that if he had followed the contrary plan, supposing always that he had some original facts worth narrating, and we are sure that such is the case, his paper would have been more acceptable to most of his readers, who probably have in their hands the greater number of the works from which he has quoted. Be this as it may, Dr F. reprobates the early use of external stimuli in internal inflammations of the serous and mucous membranes (among the latter of which we are glad to find he includes fevers), restricting their employment to the moment "betwixt high excitement and the state of collapse, which Dr Rush denominated the blistering point." He alludes with decided disapprobation to the practice so general some years ago, and occasionally pursued even at a later period in our country, of applying many blisters in fever, and keeping them open by savin ointment. He thinks, very justly, that it would be much more philosophical, where cups and leeches cannot be procured, instead of persisting with the irritating substances, to endeavour to subdue the irritation and congestion by venesection and the more soothing applications. Among these he prefers emollient and anodyne articles, which he thinks act on the nervous extremities, and secondarily, on the centres, and not by the absorption of their particles. He cites many authorities to show the efficacy of these means in a variety of maladies.

14. *Pertussis arrested by the use of Belladonna and Hydrocyanic Acid*—Dr William W. Valk of Providence, R. I., reports in the February (1831) number of the American Journal of the Medical Sciences, that he has used with great success in pertussis the combination of belladonna, ipecacuanha and sulphur as recommended by Dr Kahleiss of Germany. In a case unattended, as Dr V. thinks with pulmonary congestion, and in which "an occasional emetic was given to throw off the accumulation of mucus in the air passages," the Doctor determined to try belladonna, &c. and accordingly prepared the following formula. 1. R. Pulv. rad. belladon. gr. 1¼. Pulv. dov. gr. 2½. Sulph. præcip. ℈i. Sacch. alb. ℈ii. M. div. in chart. 20. One powder every three hours, and between each dose twelve drops of the following. 2. R. Aqua chamomile ℥ss. Syrup simplex ℥ii. Acid prus. gtt. vi. M. These prescriptions are less in the proportion of their ingredients than those of Dr K. as his are for a child two years old, he used nearly four times the above quantity for the first formula, and twice as much for the second. "For three days," continues Dr V. "these medicines were given with care and regularity, when the relief afforded being sufficiently evident they were omitted (on the 11th November), and from that time to the present (18th) the child continued perfectly well."

Dr. V. states that he has every reason to believe that these remedies will prove of singular efficacy in cutting short the progress of this distressing disease; for he joins Dr. Dewees and many others, in opinion that Pertussis has not a definite duration, and need not necessarily run a particular course.

15. *Treatment of Paralysis.*—We have already in an article of the present Quarterly Summary spoken of the use of strychnia in the above mentioned disease. We must now recur once more to the subject in order to notice the opinion of Dr Elliotson, the distinguished physician of St Thomas's Hospital, London. We quote from a clinical lecture delivered by that gentleman, and reported in a late number of the Medical Gazette of London.

"Strychnia unquestionably stimulates the nervous system, and no more; but

as paralysis frequently arises from chronic inflammation, or pressure, or change of structure, it can in such cases be of no benefit, and may do harm; and when there is effusion, although electricity may do good, I do not apprehend that strychnia can. There are cases in which the nervous part is merely in a state of torpor, and in these strychnia may be of use; but these instances are very inconsiderable. I have employed strychnia, and more frequently nux vomica, but I cannot say with any sort of satisfaction. Antiphlogistic measures, when employed in the first instance, will often cure this disease entirely; at any rate, if they are moderately carried on for a length of time, they will lay the foundation for a cure at a subsequent period. If you remove all the circumstances of inflammation and congestion in the first instance, I have no doubt, though no benefit takes place for some time afterwards, that ultimately you will do more good, and the efforts of nature will be more effectual, than if you do not employ them; and that strychnine, iodine, and electricity, will obtain the credit of the subsequent improvement often very undeservedly." This we consider sound, practical reasoning.

Dr E. speaks in the same lecture of a case of paralysis of the wrists, produced by lead. The disease was unpreceded by colic. Having found Strychnia or nux vomica beneficial in such cases Dr E. resorted to the former, and at the same time ordered the wrists to be electrified three times a day. The improvement was so rapid that Dr E. was sure it must be the electricity which benefitted him, and he therefore left off the strychnia—and the cure proceeded with equal quickness. The patient entered the hospital on the 2nd of October, and so far recovered under the use of electricity as to be able to go out to work on the 11th of November. He would have been discharged sooner, but that he had a slight attack of rheumatism in the knee.

16. *Stammering cured by the long continued use of Cathartics.*—Dr Bostock relates an interesting case of the sort, in the 16th volume of Medico-chirurgical Transactions. The patient was a boy of a robust form and a florid aspect, of a healthy constitution, and of more than ordinary activity of mind and body. When between two and three years old, and after having acquired considerable readiness in speaking, he was suddenly affected with so great a degree of stammering as to be almost incapable of uttering a single syllable. Two physicians, who were consulted, confessed their inability to propose any specific plan of treatment which might afford a prospect of success, and merely prescribed a strong cathartic medicine to remedy a plethoric state under which the child laboured. The effect of the purgative was so favourable that it was repeated three or four times, and each time with such decided benefit as to leave no doubt on this point, on the minds of either of the parent or the practitioner. The complaint, however, shortly recurred, was again attacked with the same remedy, and was again subdued. The advantage, though so decided, being only temporary, a system of diet, calculated to reduce permanently the plethoric habit and obviate the necessity for the continued repetition of the purgatives, was adopted, and rigidly adhered to for several years. The plan was found highly beneficial. By a steady adherence to it for about eight years, the complaint was kept at bay; but whenever any relaxation in the diet took place, or when the purgatives were omitted or too long delayed, symptoms of the impediment immediately reappeared. At length, when about twelve years of age, the tendency seemed so far subdued, that a relaxation of the restriction was not followed by the usual unfavourable consequences, and the boy being then at a public school, it was not easy to maintain the former discipline. For some time no bad effect ensued, but at length the complaint recurred, and was unusually obstinate, so as to require a long and severe course of purgatives, which, however, was finally successful. "During the last two years the tendency has occasionally manifested itself, but it has always been easily removed by a moderate use of purgatives, and by a temperate, although not a rigidly abstemious diet. The boy, who is now in his fifteenth year, may be said to be free from the complaint."

In this communication, Dr Bostock does not call attention to the condition of the gastro-intestinal mucous membrane before the purgative plan was adopted, or to the effects of the cathartic upon that tissue. The fear of inflaming it does not seem to have been at all entertained, and purgatives were resorted at first to correct a plethoric state, and afterwards continued empirically because they had proved beneficial for the stammering, or to change the nature of the stools. *How* they produced these effects Dr B. does not attempt to decide. For ourselves, we must be allowed to express our belief, that the purgatives acted by revulsion, and also, aided by low diet, by keeping down the excitement of the brain. Their bad effects were evidently obviated by the low diet, and we have little doubt, the same results in the cure of the disease might in all probability have been obtained more safely by low diet, blood-letting, leeches, and revulsions to the skin. It is not always safe to subject the gastro-intestinal canal to the frequent irritation necessarily occasioned by purgatives. The plan may sometimes succeed admirably, but the physician cannot be certain of this, and may in other cases not only be disappointed but do much mischief. Too much care cannot be bestowed by those who recommend a perturbative plan of treatment, in pointing out such condition of the organs that are to receive the impression of the remedies which will admit of their employment. When this is neglected, the recommendation of such a treatment may be often a source of evil, for the less experienced practitioner, who feels inclined to adopt it, will be liable to do so in cases in which the state of the gastro-intestinal mucous membrane should really contraindicate it. Under these circumstances, sufferings, and even death are liable to follow. Many, we know, will get over the embarrassment produced by the unexpected result, by talking of a sudden accession of a new disease; of some unlooked for complication; but the physiological physician,—one who has paid attention to the mode of action of remedies, and is aware of the value of symptoms, knows what opinion to form of these unexpected and new complications.

17. *Treatment of Fevers.*—The French physicians, after employing much of their time with researches into the anatomical character of diseases generally, but of fevers in particular, seem to have lately turned their attention to the subject of therapeutical indications, and the effects of particular remedies. The Parisian medical journals of the last year contain many essays which attest this fact. Among these essays we find a few which appear to us to merit a notice, and a summary of whose contents we consequently propose to lay before our readers in this and in succeeding numbers of our Journal. We commence with two memoirs on fevers by M. Dance of Paris, contained in the September and October numbers of the *Archives Générales de Médecine*. In the first essay M. Dance gives the details of sixteen cases of the disease, all of which were treated by *tonics*. They are divided into three series. Those in the first series are four in number, and all ended fatally. But it results from the history of these cases that they were submitted to treatment after the symptoms had progressed so far as to leave little hopes of a recovery, whatever plan had been pursued. Antiphlogistics were at first employed, but without success. Tonics were now substituted for these means. So far, however, from proving more effectual, they aggravated the disease; the tongue and gums became black, inflammation of the sub-maxillary glands occurred, followed by eschars in the region of the sacrum, prostration of strength and death. On dissection the mucous membranes were found thickened, softened and ulcerated to a considerable extent. The second series is composed of five cases, all of which likewise proved fatal. Antiphlogistics were also had recourse to in these to combat symptoms of general and local surirritation. As soon, however, as subsultus tendinum, stupor, prostration of strength, &c. occurred, the plan of treatment was changed, and camphor, bark and the like remedies were resorted to. These remedies, however, did not produce the least good in any of the cases, for the symptoms were aggravated either slowly or immediately, and death, in all, ultimately followed. It is evident from a perusal of these cases that the death of the patients must be attributed in great measure to

the means employed; for, unlike those of the first series, which were of the most malignant kind, these were of a mild nature. Several, indeed, appeared to be getting well, when the plan of treatment was changed. On dissection morbid conditions,—redness, thickening and ulceration of the mucous membranes were discovered. But they were not sufficiently extensive to be regarded as incurable, or as sufficient to account for the fatal termination. Besides, such was the aggravation of the disease from the tonic treatment, that we are induced to attribute to the latter a considerable agency in the production of these pathological changes in the membranes. We are led to this opinion by the circumstances, that the worst of these changes were observable principally in the large intestines; and that it was precisely on these parts that the medicines in question produced their impression, for they were administered in the form of enemata. One patient took in that way five hundred grains of camphor in one month.

The third series contains seven cases. They all terminated, like those of the preceding series, fatally. But they differed from the others in this, that the employment of tonics appeared at first to produce some amendment in the symptoms, and secondly that the pathological changes in the intestinal canal were not considerable. As regards the first circumstance, the momentary amelioration and subsequent aggravation of the disease, M. Dance remarks, that it resulted from the artificial stimulation which braces up the system for a little while, but, soon subsiding, leaves it in a state of greater inertia. The same phenomena invariably occur when stimulation is made use of in health or disease, though particularly when too powerful or resorted to under unfavourable circumstances. It wears away the power of life and aggravates the disease. It is probable that tonics appeared advantageous in these cases, only because the disease was of a mild character; for the pathological changes in the intestines were inconsiderable. If the stomach was in some cases found diseased, we have reason to believe that the effect was in greater measure produced by the tonics. But whether this be the case or not, it appears probable that in the instances before us these remedies aggravated the disease; and whether we regard the latter as produced by a local inflammation or as existing independently of this, we must conclude that tonics, so far from curing, will tend to aggravate it. It is right to state, at the same time, that in all the patients of whom we have spoken there occurred eschars on the sacrum, sometimes near the trochanters; that the ulcers produced by blisters generally became gangrenous, and that unhealthy inflammations arose in one case spontaneously on different parts of the skin. These effects cannot be attributed to putridity, for in that case the tonics, which were liberally employed, should have remedied it. Besides, as M. Dance adds, similar cases treated on a different plan hardly ever manifest these phenomena.

The second memoir contains a detail of the fourth, fifth and sixth series of cases treated by means of tonics. These cases are twelve in number, and have all terminated in a successful manner. In these instances the disease was evidently of a mild character, and would probably have got well as readily, if not more readily, without as with tonics.

In some cases of the fourth series it was indeed found necessary to suppress for a few days the use of the bark in consequence of its producing diarrhœa. Besides, unlike what took place in the preceding cases, this remedy was given alone and uncombined with camphor and acetate of ammonia. At any rate the advantage derived from bark in these cases was not so great as to lead to the use of the remedy in other cases of a similar kind. All we can allow is, that it will not do so much harm as might have been expected.

In the cases of the fifth series, the patients having recovered, the success may perhaps be attributed to the tonics employed. But in reading the details of these cases, we find that the benefit derived from these remedies is very problematical. In some, the patients were three months before recovering perfectly, and in various individuals relapses, diarrhœa, intellectual derangements were observed. In others the cure could only be referred to several critical abscesses, supervening on the lower extremities. Besides the tonics were used very sparingly;

yet in two cases, the disease, which was of the mildest kind, was greatly aggravated, and the patient recovered with great difficulty. In many, antiphlogistics were employed at the same time with tonics; and in several the cure could not be accomplished until these remedies were discontinued, and the patient put on spare diet and mild remedies.

In the patients of the sixth series, four in number, the use of tonics was preceded by that of antiphlogistics; and they were only employed for a few days and with great caution, and then discontinued in consequence of the bad effects they produced. M. Dance concludes from all that precedes—after a careful examination of the phenomena presented in all the cases he describes, that tonics, even when employed with the greatest caution, are not free from danger in the diseases in question; and that when they appear to act advantageously, the benefit derived from them is only temporary. Nevertheless, he does not pretend to condemn the use of these remedies entirely; but merely expresses the opinon, that as their proper application is a point of such delicacy, it would be more prudent to abstain from them altogether.

M. Dance next relates the histories of twelve cases of fever treated by antiphlogistics as the principal remedies. These are divided into two series—the first consisting of seven cases, the second five. All these cases ended fatally. Those of the first series terminated in the first period of the disease, as may be inferred from the short duration of the case and from the circumstance that the follicles in the intestines were simply tumefied and not yet ulcerated. All the individuals composing that series died with symptoms of cerebral derangement, although on dissection this organ presented a less degree of alteration than the intestines (except, however, in one case), this alteration consisting in simple sanguine or serous congestion of the membranes or substance of the brain. In one case, no alteration whatever was discovered in the cranial cavity. In all, the disease being inflammatory, as may be inferred from the nature of the lesions found and the symptoms observed during life, the most active means of the antiphlogistic kind were resorted to, but to no avail. The delirium did not abate in one single instance from the operation of these remedies, and was even in some much aggravated by them. Indeed, as M. D. remarks, if we judge empirically of the treatment, without any reference to the nature of the disease, we are led to conclude that death was certainly accelerated and even produced by the too free and ill timed abstraction of blood. M. D. is inclined to think that the delirium in those cases was often, when continuing after very copious depletion, rather the effect of nervous derangement than of sanguine congestion, and that it may together with the convulsions noticed have been the results of or have been aggravated by these means—circumstances which sometimes take place after very copious hemorrhages.

Some may suppose, M. Dance remarks, that in those cases the lesions, at least those in the alimentary canal, were of so serious a character, as to call for this energetic treatment, and that death occurred because the force of the disease was superior to the remedy. He finds an answer to this supposition in the cases of the second series. In all these instances death occurred and was preceded by symptoms of cerebral derangement. The pathological changes in the intestines were inconsiderable, if compared to those observed in the cases treated by tonics,—consisting, as they did generally, in a few follicular engorgements, and still fewer ulcerations. In some cases the colour of the lining membrane of the stomach was abnormal,—sometimes thickened and softened. The parietes of the intestines were in some found thinner, and the mucous membrane less villous than in a healthy state. But whatever degree of importance we may attach to these lesions (a part of which may be regarded as a cadaveric result), they cannot be considered incurable. The lungs were generally found engorged and softened at the posterior part,—the brain was found sometimes hardened and engorged,—in other cases, red and soft. The treatment was directed chiefly to the gastro-intestinal disease, and was very active. Notwithstanding this, the disease became aggravated, and finally proved fatal. "If we recollect what has been said of the small number of engorged or ulcerated intestinal follicles (morbid

alterations which constitute the principal anatomical state of the disease which occupies us at present), we shall be ready to conclude that several of the patients, whose disease have been described, would not have died had the depletion been less abundant and more judiciously directed, for it is not the use but the abuse and ill timed employment of this means, which we combat."

M. Dance promises to continue his inquiries on this subject in subsequent numbers of the Archives.

18. *Convulsions, treated by Frictions of Tartar Emetic.*—M. Debourge, of Rollot, in France, describes in the Archives Générales, a case of convulsions in an infant eight months of age, suckled by a woman who for some time had been a prey to violent grief. The gums of the child were not in a state to indicate that they were the source of irritation,—the abdomen was soft and not painful, and there were no symptoms indicating the existence of worms in the intestinal canal. Nevertheless, after resorting ineffectually to antiphlogistics, a vermifuge medicine was administered; but no worm was passed, and the disease, so far from being mitigated by it, was aggravated to such a degree that on the fourth day little hopes were entertained of the recovery of the child. Under these circumstances M. D. determined to resort to tartar emetic, agreeably to the Iatraleptic method. Two drachms of deuto tartrate of potass and antimony were dissolved in ten drachms of very hot water,and frictions with a flannel dipped in this solution were directed to be made all along the spinal region, and to the legs and thighs. After the fourth application the convulsions ceased to recur so frequently, and the attacks were of shorter duration than before. On the next, the infant had but three paroxysms,—the night following only one, which was the last. Four drachms of the tartrate were thus employed in the course of forty-eight hours, without producing either diarrhœa or vomiting. Only three pustules appeared; they were situated on one of the thighs, and did not make their appearance until after the cessation of the convulsions. The rest of the skin was only slightly red. M. D. asks whether the cure could be attributed to this therapeutic means. He is inclined to answer in the affirmative; but adds, very properly, that nature operates sometimes wonderful cures!

19. *Large doses of Ipecacuanha in Dysentery.*—Mr W. Twining, in a sensible paper inserted in a late number of the London Medical Gazette, strongly recommends the employment of "ipecacuanha in very large doses, for the purpose of relieving tenesmus and irritation, restoring a healthy state of the alvine evacuations, and in more remote stages of the disease, promoting the healing of intestinal ulcers."

Mr T. acknowledges that ipecacuanha has long been employed in the cure of intestinal profluvia, and that its utility in alleviating some of the symptoms attending those diseases seems, to a certain extent, generally admitted. But he adds, that the mode in which it is usually exhibited induces him to believe that no great reliance has been of late years placed in this remedy, although it was originally introduced to the notice of the profession in Europe, as a medicine of the greatest efficacy in the cure of dysentery. "A reference to the history of the remedy may satisfy us that it has been held in estimation in the cure of that disease, just in proportion as it was administered in large doses; and it was formerly sometimes prepared by boiling, so that the emetic properties of the root were destroyed; whereas, when given in small doses, it has been considered of less efficacy, and in severe cases is now generally looked on as an adjuvant of minor importance, the benefits appearing to belong, in a great measure, to the other remedies with which it is combined."

Mr T. recommends the remedy to be given, combined with a bitter extract, whereby vomiting is counteracted. "I have generally," he remarks, "used the gentian for this purpose, and by prescribing these two remedies in nearly equal proportions, we may, if requisite, give from six to twelve grains of ipecacuanha without often exciting nausea." "The remedy appears advisable in the early stage of almost all ordinary cases of acute dysentery; in the treatment of which

the first object (after adequate bleeding) is to clear out the bowels by a dose of castor oil; twelve hours after this has operated freely, the patient should take six grains of powder of ipecacuanha, with four grains of extract of gentian, and five grains of pil. hydrarg. in three pills, which are to be repeated every night at bed time, and at daylight in the morning; a dose of compound powder of jalap (dr. i.) is given daily at eleven o'clock in the forenoon, but if the patient be of a very irritable habit, dr. ss. will be sufficient for a dose. By these means we may soon procure a cessation of all the distressing symptoms, and it is then sufficient to continue six grains of ipecacuanha, with four of the extract of gentian, every night, giving a smaller dose of compound powder of jalap, or a moderate dose of castor oil every morning, for four or five days more."

20. *On the efficacy of Belladonna in a case in which the evacuation of fæcal matter was attended with much pain.*—Dr De Laporte, of France, relates the following case in the Journal Universal et Hebdomadaire, No. 4, 1st volume.

Mrs R. aged thirty years, and of lymphatico sanguine temperament, had experienced great difficulty in having alvine evacuations since her first accouchement, at which the application of the forceps was resorted to. She attributed her complaint to hemorrhoids, because she had suffered much from costiveness during her pregnancy. The pain during each stool was excruciating, and the patient's health much deranged. She refused to submit to an examination, and Dr De Laporte presumed from the analogy of the symptoms to those of other cases he had seen, that the patient laboured under a fissure, and a spasmodic contraction of the anus. He prescribed the application of the ointment of belladonna to the anus,—a remedy which he had already successfully employed in similar cases. The success was complete, and in a short time the patient was radically cured. Since that period she has borne two children without experiencing a return of the complaint.

The thirty-sixth number of the Medical Gazette, of Paris, contains an account of a case of spasmodic stricture of the rectum, in which the same remedy was employed successfully by M. La Borderie.

21. *Treatment of Venereal Diseases.*—Dr William Donnelly, Surgeon in the royal navy, has published in the number of the Edinburgh Medical and Surgical Journal for January 1831, the results of his observations on the above subjects, while on board his majesty's ships Hussar and Sparrowhawk. In the first of these vessels there occurred sixty-one cases of primary venereal affections, from March 1827, to the end of July 1830. The general treatment of these was rest, low diet, abstraction of liquor, and occasionally a saline cathartic. In buboes, cold applications were used to prevent suppuration; and when this result took place, with sinuses, the swellings were opened by incision, and occasionally by caustic. Excoriations were treated by sulphate of copper, or nitrate of silver. Warts were removed by the knife, and by nitrate of silver. Seventeen cases of chancre were treated with ordinary dressings, by nitrate of silver, sulphate of copper, or sulphate of zinc, occasionally without particular regimen. Twelve cases complicated with bubo, suppurating and non-suppurating, were treated by confinement and local remedies, as indicated.

Of the sixty-one cases treated in the Hussar, without Mercury, four only were affected with secondary symptoms.

Of twenty-six cases of primary venereal affections, which occurred in the Sparrowhawk, in England, Sicily, and at Naples, in which the general characters were excoriations, chancres and buboes, suppurating and non-suppurating, and which were treated without mercury, they were on an average fourteen days on the sick list, and eighteen days under treatment. In only one did secondary symptoms supervene and that two months after the cure of the primary affection, and in the remaining twenty-five, which were watched for fifteen months, no symptoms were observed to follow during that period.

Dr Donnelly remarks that among the whole number of cases treated in both

vessels, he met with six in which secondary symptoms appeared without being preceded by those which are regarded as characteristic of syphilitic action, or after those symptoms were supposed to have healed entirely. In an early part of his essay he appears to laugh, and we believe with perfect reason, at those physicians who ascribe almost all varieties of disease to a latent uneradicated syphilitic or gonorrhœal virus, and will not believe the most solemn asseverations of patients never having had venereal disease; or who, if compelled to do so, will trace the supposed poison, for its existence cannot be demonstrated to ancestors, not only of the first, but of the second and third degree. He cites an interesting case of finely characterised syphilitic ulceration of the throat, soft palate, &c. with caries, and finally loss of the bones of the nose and face, which by the gradual progress of the disease, terminated fatally about six or seven years from its commencement. The girl, who was the subject of this case, was admitted into the Halifax (N. S.) poor house, in an early period of the disease. She was then between twelve and thirteen years of age, and with a hymen large and entire, and without any genital affection. Neither mercury nor any other means arrested the slow but regular progress of this disease. If it were syphilis, Dr D. asks, whence was the virus derived? And if syphilitic, why not controllable by its assumed antidote, mercury?

Dr Donnelly presents two tables showing the number of patients treated for syphilis, in the Bermuda and Halifax hospitals; in the former, from 1809 to 1829 inclusive (excepting 1816,–17–18). In the second, from 1809 to 1819 inclusive. They were all treated with mercury, and the results, when compared with those obtained by Dr D. in the vessels we have mentioned, are far from being satisfactory. The average period, for example, in 348 cases, before a cure, invalidation, or death, took place, was sixty-one days,—the number of cured being 272, that of invalidated or dead being 68. By examining, on the other hand, the table presented of the patients in the Hussar and Sparrowhawk, we find that the others were very nearly three times as long under treatment, and three or four times longer on the sick list. From all these circumstances Dr D. concludes, that "as far as shortening the time of cure is important, the advantage is much in favour of the non mercurial treatment, which is at least gratuitous, where sores heal without its aid, whilst the inconvenience to the patient and service is infinitely less. With regard to secondary symptoms he is inclined to think, that five out of eighty-one is a smaller proportion than would have followed full mercurial courses in them all, or perhaps would succeed to such a liberal use of mercury in any equal number of persons as happened to some of the secondary cases of the Hussar and Sparrowhawk." It must be remarked here, that most of the cases of secondary symptoms occurred in individuals who had been salivated for complaints not syphilitic; from which we may conclude that these symptoms may be the consequence of the influence of mercury, as readily as the syphilitic poison.

IV. MEDICAL JURISPRUDENCE.

22. *Alleged poisoning with Arsenic. Counter Report.*—In our last number we gave the details of a report containing experiments, and the opinions deduced therefrom, by a board of medical jurists, consisting of a physician, a surgeon, and an apothecary, on the case of a person dead under suspicious circumstances. The report concluded by the board affirming their belief, that the stomach and intestines of the deceased contained foreign matter, which, by its colour, consistence, and crackling on pressure, seemed to resemble the *yellow sulphuret of arsenic* or *orpiment*. In consequence of this report, two persons suspected of being guilty of the administering the poison were arrested and put in prison, but the evidence not being deemed sufficient, a new report was required. M.M. Rostan, Marc, Orfila and Barruel, were accordingly entrusted with this task, and so soon as their opinions were ascertained, the accused were set at liberty.

The demand made of this commission, consisting of the above named gentlemen, was, 1. To examine the medico-legal report made at D——— the 3d July 1829, by M.M. in a case of suspected poisoning, and to give their opinion respecting it.

2. To submit to chemical analysis a part of the various matters which made the subject of the report; to prepare a statement of their labours, and, at the same time, to give their opinion respecting the experiments of M.M. the nominated board of inspection at D———.

We omit the preliminary formalities of expression in the report of the new commission until they state that, after having been duly sworn, they received the following from the commissary of police.

1. The report of the board at D———.

2. A box having the seal of the king's attorney.

No objection is made by the new commission to the first part of the report as contained in our last number. But as to the chemical experiments of which the second part consists, as they are not deemed to prove conclusively that the matter, found in the digestive tube of the deceased, B——, was really yellow sulphuret of arsenic, the commission think it adviseable to examine for themselves, with all possible care, the portion of this matter now transferred to them, and a part, of course, of that on which the board at D——— had experimented. The box above mentioned was accordingly opened in the presence of the commissary of police and found to contain,

1. A small jar, labelled, *A piece of stomach on which some yellow points are seen.*

2. A parcel enveloped in paper, on which was written, *Dried stomach from which have been taken yellow particles.*

3. A little flask, closed with a ground glass stopper, and labelled, *Mucous matter contained in the stomach, with a little alcohol.*

4. Finally, a flask closed like the former, and labelled, *Matter contained in the small intestines.*

The commission proceeded on successive days to the examination of the above substances.

1. As regards *the piece of stomach on which some yellow particles were observed,* the commission found some small abrasions of the mucous surface; but they could not, after the most careful inspection, aided by a microscope, discover any thing resembling yellow sulphuret of arsenic; they only find some very minute white particles, which being taken up on the point of a penknife, and put on blotting paper, gradually melted on the application of a mild heat and penetrated the paper, rendering it at the same time transparent—evidences sufficiently clear that these small white points were nothing but fat. This part of the stomach was soon dried by exposing it for some hours to the sun's rays. It was then divided into small pieces, and gradually projected into a crucible of platinum, containing nitrate of potassa in fusion; these were soon rapidly destroyed by entire combustion. The residue was white and fused. Put into distilled water, it was soon entirely dissolved, with the exception of a small portion of the phosphat of lime. The solution when filtered was limpid and colourless; it was treated by a slight excess of nitric acid, and then evaporated to dryness; in order to drive away the nitrous acid and the excess of nitric acid, the saline residue was dissolved in water, put into a matrass, and boiled for half an hour. A stream of sulphuretted hydrogen being in the mean time passed through it, the liquid remained perfectly colourless, and the addition of hydrochloric (muriatic) acid did not cause any precipitate.

The inference from this experiment is, that the portion of stomach, examined as above, does not contain the slightest portion of arsenic.

The alcoholic fluid in which the stomach had been preserved was poured out and passed through a filter: separated from some floculi of animal matter, and treated by excess of sulphuric acid, so far from acquiring any colour, it lost the amber hue which it before exhibited. Hence, it is evident, that this fluid does not hold in solution the smallest portion of a metallic poison.

2. Examination of the matter designated as *dried stomach, from which had been removed yellow particles.*

This viscus, by desiccation, had acquired a brown colour, and was thinner than natural and in some parts as thin as parchment. On examining its internal surface through a microscope some white particles were seen having the appearance of very small saline agglomerations. These small points are only broken with the nail pressed on the membrane itself and leave a small white mark. Portions of the membrane on which these white points were found in large quantities were detached and placed in a flask and boiled during fifteen minutes with distilled water. The filtered liquor, being perfectly colourless, was treated with an excess of sulphuretted hydrogen but did not undergo any change of colour; finally, the tongue being applied to some of these white particles on another portion of dried stomach did not detect any disagreeable taste.

These two experiments evidently show that these white points are not part of any metallic salt nor of poisonous vegetables, these last always having a bitter taste.

3. Examination of the *mucous matters contained in the stomach, together with a little alcohol.* This matter is viscous and contains floculi of a yellowish grey; diluted with an equal quantity of water and shaken up and then allowed to rest for ten minutes, the whole was poured out on a filter and nothing remained adherent to the flask any way resembling mineral substances. The floculent matter which remained on the filter was put apart, and the filtered liquid being treated by an excess of sulphuretted hydrogen, and some drops of hydrochloric (muriatic) acid exhibited no change of colour; whence it follows clearly that it exhibits no trace of an animal poison.

4. Examination of the *matters contained in the small intestines.* These matters consist of a brown fawn coloured liquid, of a thick consistence and very turbid. When the flask was uncorked there was a great emission of gas of an insupportable fetor. This liquid, weighing about two ounces, was diluted with the same quantity of distilled water and freely shaken up. At the bottom of the flask there was then observed a number of small points, weighty, and of various colours. On being transferred to a watch crystal, they were several times washed, and the water of each washing was poured on the filter with the first liquid.

Examination of the *granulated matter.* This matter was composed of the union of black molecules, and of yellow ones with a slight brilliancy, and of white molecules. Heated in a matrass with distilled water to the point of ebullition for ten minutes, it underwent no change of volume or aspect. The liquid was entirely limpid. Treated with sulphuretted hydrogen and some drops of hydrochloric acid, it underwent no change of colour.

On examining with a microscope the matter which was not dissolved and which had been dried, the black molecules appeared to consist of coarsely pulverised charcoal; the yellow ones had the semblance of a yellow calcareous sand, and the white molecules appeared to consist of white sand.

All these little grains, like particles, being introduced into a very small tube closed at one end and heated, there was disengaged a vapour having both the odour of fat and of animal matters when burning. The tube was carried to a red heat, then cooled; but the matter did not seem to be diminished in volume; it was of a fine black colour. At the middle part of the tube, a small band of a yellowish brown colour was formed, having the appearance of a pyrogeneous oil. The tube being cut in two by a file, the black residue was extracted, and when brought to a red heat in contact with the air, it became white without any sensible change of volume. The yellowish band condensed in the middle of the tube was treated by very weak ammonia, but was in no wise dissolved. The liquid, having been saturated with hydrochloric acid, did not give any deposit. Finally, the tube opened longitudinally, and, heated on burning coals, did not emit any arsenical odour.

The liquid from the intestines, from which the granulated precipitate had been separated, was filtered, and, having been treated by the method fitted to detect the smallest traces of arsenic, gave no evidence of any such trace.

The filters, on which the floculent matters remaining after filtration were deposited, 1, from the liquid in which had been preserved *the piece of stomach exhibiting some yellow points;* 2, from the filtration of *mucous matters contained in the stomach;* 3, from *matters contained in the small intestine*, were all projected in very small pieces at a time into a platinum crucible containing an excess of *nitrate of potassa* in fusion, and in which they were very soon burnt. The residue of this combustion must necessarily contain in the state of *arseniate of potassa* all the arsenical products whatever they may have been, if those matters had contained the slightest portion of this metal. This residue was treated by distilled water, in which it was dissolved with the exception of a sediment, which on examination was found to be formed exclusively of a mixture of phosphat of lime, resulting from the animal matters, and silica coming from the fibres. The filtrated liquor was perfectly limpid and colourless; it was strongly alkaline. A small portion of this liquor treated by the nitrate of silver gave rise to a precipitate of a white with a slight red shade. This precipitate treated with nitric acid was in great part dissolved with a slight effervescence: the portion which was not dissolved was *chloride of silver.*

A slight excess of nitric acid was added to the portion of the remaining liquid. There was a disengagement of nitrous acid. Evaporation to dryness was now carried on, in order to drive away all the nitrous acid and the excess of nitric acid, and the residue, being treated by water, was completely dissolved, with the exception of some floculi of silica which were separated by filtering. This liquid was boiled for half an hour, and, during all that time, it was traversed by a current of sulphuretted hydrogen gas, after which were added two or three drops of nitric acid which produced neither any change of colour nor precipitate.

It results from the operations described above, that not only did the new commission not find any trace of arsenic in the different matters submitted to their inspection and experiments, but they were also unable to detect any other poisonous substance.

The commission next proceed to express more formally their difference of opinion with the former board at D———, and to comment critically on the inferences of these latter. The fracture or crepitus, on pressure with pincers, spoken of by the board as one of the evidences of the yellow particles being yellow sulphuret of arsenic, is not an evidence in fact, since the sulphuret in question, when natural, is always seen in impalpable molecules; and when artificial, it is formed of laminæ adherent to one another, and so flexible as to be divided with difficulty; and when we take one of these laminæ it will twist rather than break, and when broken, it is done without noise.

The gentlemen at D——— ought, says the new commission, to be aware that the reagents of copper, such as the ammoniacal sulphat, are, of all those known, the least fitted to test in any liquid the presence of the oxide of arsenic. In fact, if we pour some drops of the ammoniacal sulphat of copper into distilled water, it is decomposed; the liquor becomes turbid, and a blue precipitate is formed; and if to the distilled water we add a drop or two of a yellow liquor, such as a solution of the chromate of potassa or the tincture of saffron, in such a manner, however, that the water shall be barely discoloured, the ammoniacal sulphat of copper is equally decomposed, and the turbidness shows of a fine green. But if it be filtered, the precipitate which remains on the filter, when washed, is of a clear blue colour. Now we cannot admit as proof of the presence of arsenic in the matters examined by the board at D———, the different green colours, which they obtained by trial with the re-agents of the salts of copper, since they merely say that they have obtained a green colour with these re-agents; had they perceived a precipitate, they would have collected it and ascertained its colour.

As to the ammoniacal nitrate of silver employed by them, it cannot be admitted as showing the presence of the oxide of arsenic, for the arsenite of silver is excessively soluble in ammonia, and besides, this arsenite is of a yellow colour, and the arseniate alone is red.

The examiners at D——— say, " that some of these particles, which in their

opinion are yellow sulphuret of arsenic, when boiled with distilled water became white." Now yellow sulphuret of arsenic, whether natural or artificial, undergoes no change in boiling water; the natural sulphuret does not even lose its brilliancy. It is evident, that if the yellow particles on which these gentlemen operated became white by boiling in water, we are to conclude that they are not fragments of the yellow sulphuret of arsenic.

Finally, the experiments made by these gentlemen on the matters collected in the small intestines (see page 232 of our last number) lead to the following reflections. Let us admit the hypothesis that the matter contained sulphuret of arsenic. One of two things; either they added enough of nitrate of potassa to burn completely the matter obtained from the intestines, or they did not add enough.

Under the first supposition, as the experiment was made in a crucible, consequently in a high heat, the sulphuret of arsenic would have been converted into a sulphat and arseniate of potassa; in this case, the reagents of copper would have been precipitated of a blue colour, and the nitrate of silver of a red, for it is thus this salt acts on these reagents.

Under the second supposition, that there was not enough of nitrate of potassa introduced, the carbonate of potassa, proceeding from the decomposition of the nitre by a portion of organic matter, would have decomposed the sulphuret, and the excess of charcoal would have decomposed the oxide of arsenic, and the arsenic would be completely volatilized in a metallic state. Whichever supposition we admit, as we are assured that the matter in question contained but a small portion of sulphuret of arsenic, since there *were seen but very small pieces through the microscope*, the liquid formed by the solution of the cindery powder contained necessarily in either case, some carbonate of potassa; now, this salt precipitates abundantly the nitrate of silver, and this precipitate, which is of a dirty white, is carbonate of silver, which, as well as the arseniate of silver, is excessively soluble in nitric acid.

On the manner in which the matter, taken from the intestines and collected on the filter, was examined by the board at D——(see p. 233, last No.), the new commission make the following comments. It is conceivable enough that any matter whatever, which should contain sulphuret of arsenic, should, by being burned in oxygen gas, be converted into sulphurous acid and oxide of arsenic, which produces white vapour, but they know that this white vapour is speedily deposited on the sides of the vessel in which the combustion had been performed, and that there is no need of a current of sulphuretted hydrogen for that purpose; also, that the sulphuretted hydrogen causes this oxide to pass to the state of a sulphuret of arsenic, and *not partially so;* but we cannot well understand why the yellow liquid at the bottom of the matrass through which had been passed a current of sulphuretted hydrogen, should not have precipitated the black colour of the salts of copper as well as of the ammoniacal nitrate of silver. But in strictly supposing that this liquid did not contain sulphuretted hydrogen, it would be sufficient for the liquid to be of a straw colour, in order to explain the change of the salts of copper to a green colour.

A conclusive experiment, say the commission, and on account of its importance, we repeat the expressions from our last number, would have been the reduction of the small yellow particles into metallic arsenic,—an experiment not attempted by the gentlemen at D——, although we know that it may be performed on exceedingly minute quantities.

In conclusion, the new commission have only been able to discover one fact in the chemical part of the report of the board at D——, which might have led its authors to suppose that they were in the way of detecting the cause of poisoning; it is the alliaceous odour disengaged from one of the small yellow points obtained by them from the stomach, when placed on a burning coal; but all the others which follow do not by any means strengthen the grounds of their first opinion. The counter report is signed, Rostan, Marc, Orfila, Barruel.

The court having decreed that there was no ground for farther proceedings, the accused were set at liberty.

V. MATERIA MEDICA.

23. *Action of Acids upon Salicine.*—In a former number of the Journal we stated that salicine had been found not to possess alkaline properties. It appears, however, from the further researches of M. Peschier of Geneva, detailed in the *Journal de Chimie Médicale* for November last, that, though not in its ordinary state alkaline, it is rendered so by the action of acids, with which, after having undergone the change, it unites to form crystallizable saline compounds. In order that this effect may take place, it is requisite that the acid should not be very greatly diluted. We give an abstract of the results obtained by M. Peschier.

Nitric acid poured on salicine speedily dissolved it. In about two hours the liquid was converted into a yellowish white, porous mass, in which, after sometime, acicular crystals were observed to form. Having been exposed to the air for a few days, the mass assumed a rosy tint, which gradually passed into a lively red; and the same phenomena were always exhibited, however various was the quantity of acid employed. When a small quantity of acid was used, the product, upon being dissolved in water, was found to have no action on blue test paper, indicating the neutralization of the acid. The inference was that the salicine had undergone some change of composition by which it was rendered alkaline, and had then united with the nitric acid to form a nitrate. This salt was obtained in a separate state by evaporating the watery solution to the consistence of a syrup, then dissolving the nitrate in alcohol, and concentrating the alcoholic solution.

Concentrated sulphuric acid immediately imparted to salicine a very lively red colour, which passed to a wine red, and even to a dirty rose colour upon the addition of too large a proportion of the acid. Diluted with five or six parts of water, the acid produced no change of colour in the salicine, but entered into combination with it in the same manner as when concentrated. By diluting the solution with water, saturating the excess of acid with carbonate of soda or carbonate of potassa, then evaporating to the consistence of syrup, and treating the residue with alcohol, a spirituous solution of the sulphate of salicine was obtained, which upon being concentrated yielded the salt in a crystalline state. This result was obtained whether the sulphuric acid had been originally added to the salicine in a concentrated or in a diluted state. In the former case the crystals were in the form of silky prisms, diverging from a common centre, and had a brownish colour; in the latter they took the shape of a prismatic vegetation, climbing up the sides of the vessel, and were white. The taste of the salt was very bitter.

Muriatic, phosphoric, and acetic acids formed salts in like manner with the salicine.

Having determined the existence of these saline compounds, M. Peschier endeavoured to separate the alkaline basis. For this purpose he separated the sulphuric acid from a solution of the sulphate by means of baryta-water, and having satisfied himself that no baryta remained in the solution, tested its character by means of turnsole paper previously reddened by an acid. The blue colour of the paper was restored, proving the existence of an alkaline substance in the solution. By evaporation, this substance was obtained in the state of prismatic crystals accompanied with white masses different from those presented by salicine, and having a very bitter taste without the aromatic flavour of the willow.

That the acids should effect such a change in salicine ceases to be remarkable, when we consider, that by the same agents sugar and gum are converted into acids, and starch into sugar.

24. *Quantity of Cinchonia and Quinia in the different kinds of bark.*—From the same number of the *Journal de Chimie Médicale* we extract the following results obtained by Mr Viltmann of Osnabruck.

100 parts of Carthagena bark yielded 3.30 parts of alkaline matter;

100 parts of Huanuco bark—an inferior variety of pale bark usually confounded in this country with the Loxa bark—3.5 parts of cinchonia;

100 parts of the "quinquina royal"—the royal yellow or Calisaya bark—5 parts of quinia; and

100 parts of the red bark, 6. parts of quinia and cinchonia.

The proportion of quinia in Calisaya bark, as above stated, is considerably greater than that obtained by Pelletiero Caventore. In the 5th volume of this Journal, p. 475, the results obtained by these chemists are stated to be 2.9 per cent of sulphate of quinia from good Calisaya bark, and 1.5 per cent from the inferior kinds.

25. *Magnetism a remedy in various diseases.*—The following is the substance of a note transmitted by Dr Keil to the French Institute at its sitting of the 20th of September, and published in the number above referred to of the *Journal de Chimie Médicale.* The Doctor has discovered a mode of preparing magnets much more powerful than any hitherto in use. The strongest magnets before known were not capable of supporting more than 40 kilogrammes (about 107 lbs. troy), and weighed themselves from 40 to 45 kilogrammes. That exhibited by the author to the academy had a power of 150 kilogrammes, and weighed only 20.

In his practice as a physician in Germany, Dr Keil has made numerous experiments with very strong magnets; and has obtained the most happy results in a multitude of *nervous* diseases which had resisted the ordinary therapeutical methods. By simply passing his magnets over the parts affected, he has succeeded in curing *gout, tic douloureux, recent and chronic rheumatic pains, epilepsy, spasms in the stomach, hooping-cough, nervous weakness, rheumatic deafness, swelling of the cervical glands, menstrual irregularities, headach, convulsive muscular contractions* and *toothach.* We give the list exactly as we find it.

As magnetism acts only upon the nervous system, it is without effect when the disease depends upon organic lesion. The author has also sometimes, but very rarely, found it ineffectual where no lesion was apparent.

Numerous acute nervous diseases cease instantaneously upon the employment of the magnet. In chronic cases it is often necessary to repeat the application once or twice a day, for several days. Should the disease resist the remedy for a month, a cure by the magnet is hopeless.

In healthy individuals uneasy sensations, and even vomiting, may be produced by passing the magnet over the surface in suitable directions.

26. *Economical method of preparing Distilled Water.*—In the preparation of distilled water, according to the ordinary process, it is necessary to throw away a very considerable portion of that which first comes over, in consequence of the carbonic acid which it contains. At least one-sixth of the water must be distilled, under ordinary circumstances, before it comes over sufficiently free from carbonic acid to produce no precipitate when added to the solution of sub-acetate of lead. There is consequently a loss of time and an expense of fuel in the process, which it is desirable to avoid. M. Guéranger, an apothecary of Mans, in a note upon the subject communicated to the *Journ. de Chim. Médicale,* states that he attempted to drive off the carbonic acid by boiling the water for a short time with free exposure to the air, before commencing the distillation; but without success, as the first portions, which he obtained upon submitting the boiled water to the distilling process, were considerably impregnated with the gas. It then very naturally occurred to him that the acid might be fixed by means of lime, and the whole product of the distillation be thus rendered fit for use. He accordingly mixed some lime with the water previously to distillation, and examined the result. The first product rendered the solution of sub-acetate of lead very slightly turbid, owing probably to the carbonic acid contained in the air of the apparatus; for after a very short continuance of the process the water

was obtained entirely free from the acid, and the whole was sufficiently pure for use.

27. *Dangerous effects from the external application of Stramonium.*—The following case is highly instructive, and will serve to show that the practice, so much in vogue among the vulgar in this country, of surrounding the wrists of the sick with the leaves of the Jamestown weed, may not always be so innocent as we are apt to suppose it. We extract the case from the *Journ. de Chim. Médicale* for December 1830. One of the workmen in a distillery fell into a boiler containing hot water, from which he was withdrawn with both his legs scalded as high as the knee. The leaves of the Datura Stramonium were applied by a village practitioner, over the injured part, with the desired effect of calming the violence of the pain; and when the patient was visited by Dr Borzenkof, he was found in a favourable condition. The doctor directed that the blister which had formed should be opened, and the parts dressed with Goulard's cerate. Scarcely, however, had he left the patient, when the empyric prescribed a new application of stramonium upon the denuded surface. The patient was thrown into a state of delirium, approaching to madness. His mouth became extremely dry, his eyes fixed and without lustre, his senses much blunted, his pulse intermittent and almost insensible. The empyric, mistaking the condition of the patient for an attack of fever, employed several exciting remedies. Such was the state of things on the second visit of M. Borzenkof, whose astonishment at the change ceased when he beheld both the limbs surrounded with the thornapple leaves. He instantly caused the dressings to be removed, the sores to be well washed, and Goulard's cerate to be again applied. The patient again became calm; but he was so feeble that at every dressing he complained of giddiness such as is felt by a man recovering from a state of drunkenness. After some time he was perfectly cured; but there is no doubt that he would have fallen a victim to ignorance had not the poisonous action of the narcotic been seasonably arrested.

28. *Hydrocyanic Acid in Cheese.*—It appears from the experiments of Dr Witling that the poisonous effects, which are occasionally experienced from cheese, do not necessarily depend upon extraneous matters introduced, but may arise from the spontaneous development of hydrocyanic acid in this article of diet. When exposed to the action of water and of the sun, cheese after a short time exhales an ammoniacal odour. If now treated with alcohol, it affords a tincture, which, upon distillation, yields traces of hydrocyanic acid. This acid appears to exist in the cheese in the state of hydrocyanate of ammonia, and is not found at a more advanced period of the fermentation. Those spontaneous changes, by which it is developed, arise from the presence of moisture; and cheese never acquires poisonous properties, if, in its preparation, care has been taken to drain it well, to salt it sufficiently, and to dry it at a moderate temperature. These remarks are extracted from a note by Julia Fontenelle, in the *Journ. de Chim. Médicale*, for December 1830.

29. *Savine employed in Uterine Hemorrhage.*—Though savine has long been known to exert a powerful influence over the uterine actions, and has been employed in amenorrhœa, and in leucorrhœa, we are not aware that it has attracted general notice as a remedy in menorrhagia. In the *Revue Médicale* for October last, we find a note extracted from a German Journal, recommending it highly in passive uterine hemorrhage, and ascribing the credit of the application of the medicine to Dr Wedekind. Dr Feist has found it especially beneficial in cases in which the hemorrhage has been of long standing, and the blood thin, discoloured and fetid. The formula employed by Dr Feist is the following:—*Take of powdered savine leaves three drachms; extract of savine two drachms; distilled oil of savine a scruple; mix and divide into pills, each weighing three grains.* These are to be taken three times a day in the quantity at first of four pills, afterwards five, and ultimately as many as ten. A case is related in which the favourable effect of savine was very obvious, but we have not room to copy it.

30. *Hyoscyamus Niger.*—We are happy to be able to express our acknowledgments to the London Medical and Surgical Journal for the following facts contained in a communication read by Jos. Houlton, Esq. in December last, before the London Medico-Botanical Society. Mr Houlton asserts, contrary to what is stated in most modern works on botany and materia medica, that hyoscyamus niger is a biennial plant, and is in a fit state for medical purposes in the second year only of its duration, when in flower, or according to the excellent general rule of the Royal College of Physicians, *postquam flores expassi fuerint, et antequam semina maturescant.* The leaves at this period differ very much from those of the first year. They are cauline, sessile, very clammy and fetid, and contain a large proportion of extractive matter. The leaves of the first year are radical, petiolated, with very little clamminess, without fetor, and contain considerably less extractive matter than those of the second year. The tincture from the mature leaves is of a deep greenish brown and not clear; the tincture from the first year's leaf is much paler and clearer; the difference in their relative strength is great. Now the effect of a severe winter is to destroy the plant; and thus cause the market on the following season to be supplied with leaves of the first year's growth, and of much inferior virtues: and it is not improbable that the immature leaves are often employed in ordinary seasons from ignorance of their comparative inertness. We can thus readily understand why the hyoscyamus is of such variable strength; and may reasonably hope that with a knowledge of the cause, the defect will be remedied. As the plant, though not a native of this country, has been introduced, and is capable of being cultivated, it is desirable that we should take the remedy into our own hands, and cease to depend upon the markets of England for a supply.

We find that the plant is stated to be *biennial* in Bigelow's American Medical Botany.

31. *Croton Oil as an External Irritant.*—We have received from London a monograph upon the croton tiglium and its expressed oil by Michael James Short, M.D., from which we extract the following remarks. Having given a botanical account of the plant, and a history of the medical application of the oil, with cases illustrative of its highly beneficial effects as a purgative in a variety of diseases, the author goes on to say that he has employed it externally as an irritating liniment. "I am not aware," he observes, "that its application in this way has been practised or noticed either in this country or India. I am, however, decidedly of opinion, from the experience I have had of its efficacy as a counter-irritant, that it is preferable to all those now in use; superior to the cantharides in the quickness and certainty of its operation, and in the permanency of its effects; to the ointment of tartarized antimony, for the same reasons, as well as for not producing that excessive pain and constitutional irritation which usually attend the operation of the last preparation; and to the common sinapisms, because it stimulates the skin much sooner, diffuses more warmth, and can be better regulated as to the extent of its effects. I have employed it in this way in acute and chronic rheumatism, in gout, in tic douloureux, in glandular and other indolent swellings, and in all cases with the most satisfactory result. In cases of pulmonary affection, where it has been desirable to keep up a permanent irritation on the chest, I have found it to answer the purpose infinitely better than either the lyttæ or the tartarized antimony, producing neither the inconvenience nor uneasiness of the one or the other. I am of opinion also that it is greatly superior to and more beneficial in its effects than these medicines, from the nature of its operation; as it is pure pus which is secreted from the skin in consequence of its application, which secretion can be extended *ad libitum*, each pustule thus becoming, in some measure, a separate issue; whereas it is merely serum which is discharged when a blister is applied, and there is little discharge at all from the ointment."

Making all due allowance for the paternal partiality of the above remarks, we can readily admit that the author has made a valuable addition to our stock of

external irritants. We fear, however, that the high price of the oil in this country will prevent its general use.

32. *Rhatany used to prevent Abortion.*—A case is recorded in the *Journal Univ. des Sciences Médicales* (t. LIX, cahier 175), in which this astringent was successfully used in a case of threatened abortion. The lady had previously twice miscarried, notwithstanding the use of blood-letting with perfect rest. On the third attack, after the occurrence of a slight hemorrhage from the uterus, she was put to bed, but instead of being bled was placed upon the use of rhatany, of which she took in less than sixty hours one hundred and thirty-two grains, with twelve grains of alum. The hemorrhage was arrested, under this treatment, on the second day, and the patient afterwards passed through the regular period of gestation, and was delivered of a healthy child. The author, M. Chaufard, physician to the hospital at Avignon, appears to be in doubt whether to attribute his success in the case to the use of rhatany, or to the omission of blood-letting.

33. *Anti-epileptic powder of Dr Kahleis.*—In the *Archives Generales* for October last, we find the following formula extracted from the *Journal der practischen Heilkunde*, recommended highly by Dr Kahleis as a remedy in epilepsy.

Take of the root of mugwort, *artemisia vulgaris*, two ounces; make a strong decoction with water, and strain; to six ounces of this decoction add ten grains of powdered nitrate of silver; separate by filtration the precipitate which forms, dry it in the shade, and preserve it for use. The powder thus obtained should be mixed with a sufficient quantity of sugar, and then divided into sixty equal parts. One of these is to be given as a dose morning and evening.

VI. SURGERY.

34. *Amputation at the Knee Joint.*—Injuries of this joint have been regarded as among the most serious accidents to which the human frame is liable, and hence surgeons have discouraged any operation involving the cavity and ligaments of this articulation unless absolute necessity require the interference. The reasons have not been very evident, and some authorities have actually denied that any peculiar danger existed. M. Velpeau, surgeon of l'Hôpital de la Pitié, has lately embraced the same opinion; and, at page twenty-four of the twenty-fourth volume of the Archives Générales for September 1830, has detailed a history of the amputation of the leg at the knee joint, and the facts which have induced him to recommend and perform it. He states that the ancients always amputated at the joints, as they were ignorant of any suitable means to arrest hemorrhage, &c., but that since the division in the continuity of the limb has been adopted, the former operation has usually been condemned by writers with the exception of J. L. Petit, De Hoin and De Brasdor. Of late, M. Blandin, in the Dictionaire de Chirurgie Pratique, has revived the arguments of Brasdor in its favour. The operation has, however, been performed occasionally; and the author quotes two cases from Petit, and one from Hoin, in which the operation was successful; and it would appear that our countryman, the late Dr Nathan Smith, deserves the credit of first reviving attention to this operation, and of insisting on its comparative safety and advantages. A case, in which he successfully operated in April 1824, is alluded to in this Journal, vol. i. p. 456, and is quoted by M. Velpeau. In the same year, M. Richerand operated in the same manner, and with success, and other cases are quoted from authors. M. Blandin has detailed in the Dictionaire Pratique, &c. a case of his own; the patient, however, died, not from the state of the cartilages, ligaments or bones of the condyles, but from phlebitis. Hence it appears, that in nine authentic cases, one only has perished. M. Velpeau relates some similar operations which he has performed. The first was in a case of necrosis of the tibia, in which the head of

the bone was also involved. The patient was nineteen years old, and submitted to the operation on the 14th of January 1830. "No accident supervened, and, although there remained a transverse line an inch in breadth which the flaps could not cover, the cicatrix was complete at the end of two months. Now, this patient enjoys good health, and can sustain the weight of the body with the same facility as if he had submitted to an amputation in the continuity of the thigh." A second operation of amputation at the knee joint M. Velpeau executed in a case of gangrene of the leg from a compound fracture, accompanied by much irritation, fever and delirium. There was fever for two days after the operation demanding venesection once or twice. Afterwards there was no accident until the sixth or seventh day when an erysipelas supervened; this was successfully managed, and by the sixtieth day the cure was complete. The operation was performed about the 30th of May 1830. A third case of the same kind is also quoted, in which a young man, who was operated on in childhood, was using with great convenience a wooden leg, and could walk as easily as if the amputation had been below the knee.

M. Velpeau attempts to set aside the objections advanced. 1. The dangers, supposed to arise from the exposure of large osseous and cartilaginous surfaces are, he says, chimerical. The bone is not exposed, and "the cartilaginous plate which covers the condyles is a protecting lamina altogether insensible, which will remain for weeks exposed without the least inconvenience resulting; the pretended synovial membrane with which Bichat has gifted it, not existing, it is absolutely impossible that it should inflame." M. Velpeau says, that these observations respecting cartilages and their synovial membranes he has proved by a series of observations which will make the subject of another work. We will therefore wait for these facts, merely observing that the ideas are opposed to analogy, to past anatomical examinations, and to the experience of surgeons (Dr Smith included), who speak of adhesions of synovial membranes covering cartilages. Dr Smith says, "the synovial membrane (after amputation at the knee joint) immediately assumes the adhesive inflammation, and union is speedily effected. The stump is more useful, as the lower part is formed by the patella, which becomes anchylosed to the former."

2. The idea that an enormous wound is produced, which cannot be covered by the soft parts, is erroneous, as the whole operation consists in a division of the skin and ligaments, and if two or three inches of flap be preserved, the whole can be covered. 3. It is also a mistake to suppose that any disadvantage results from exposing tissues which do not readily inflame, and cannot speedily form a solid cicatrix as in a fleshy portion of the limb. The skin is fully adequate for the formation of a good cicatrix. 4. The idea, that it is more painful and difficult than amputation of the thigh, is no better substantiated than the preceding objections; and 5. The accusation, which is most insisted on, that the patients, after the cure, will be left in the same state as those who have suffered an amputation of the thigh; that is, they will be forced to walk, not by means of a wooden leg, but by means of a thigh piece (cuissart) in addition, is refuted by experience, which proves that the wooden leg answers every purpose.

The advantages of amputating at the knee joint, rather than on the thigh, are many. After amputation of the thigh, the point of support must be at the ischium; the motions at the hip joint are almost annihilated; progression is effected as if the coxo-femoral articulation was anchylosed. After disarticulation at the knee, on the contrary, the point of support is at the lower extremity of the femur, the thigh preserves all its motions, and the patient is in the same state as if he had suffered an anchylosis at the knee joint. The wound in this last operation interests almost solely the skin; it involves no bone, no aponeurosis; the surface to be covered is convex, regular, free from all asperities, and there is no fear of muscular contraction: in the former operation, the solution of continuity involves the large aponeurotic covering of the limb and its numerous concentric laminæ, numerous and large muscles, a bone which easily becomes denuded, the division of which is accompanied with a commotion not free from danger, and finally, all the connecting cellular tissue. At the knee, a single artery of any magnitude is divided; on the thigh, numerous vessels besides the main artery must be secured.

As to the method of operating, J. L. Petit advises the patella to be removed, but Velpeau observes it should always be preserved; the muscles draw it up and very soon fix it above the condyles where it neither retards cicatrisation, nor the subsequent utility of the stump. Dr Smith, as above noted, says the patella becomes anchylosed to the lower part of the femur.

M. Velpeau purposes to divide the skin by a circular incision, two or three fingers' breadth below the patella, and in dissecting the flap upwards, to preserve as much of the cellular and fatty tissue as possible; then to divide the ligament of the patella and afterwards the lateral ligaments. The surgeon should then separate the osseous surfaces by slightly bending the leg, detach the semilunar cartilages, divide the crucial ligaments and finish by a transverse incision, on a line with the reversed integuments of the vessels, nerves, and gastrocnemii and popliteus muscles. The hemorrhage being arrested, the flap is to be brought down and made to approach from side to side, so as to meet in a line corresponding to the sinuosity between the condyles.

The integuments will thus cover the condyles completely, and there will be little disposition to retract, and, as four muscles are only concerned, little liability to inordinate suppuration, and the ligatures will appear at the most dependent portion of the wound.

In the eighth number of the *Journal Hebdomad: November* 1830, is published the report of M. Larrey on behalf of the committee of the Academy, on M. Velpeau's memoir, and also critical and explanatory remarks of M. Velpeau on the report. We can only observe that the committee conclude, that in recent injuries, the disarticulation at the time is not to be preferred to the amputation through the head of the tibia where this is practicable, and where this cannot be resorted to, it is also not preferable to amputation of the thigh. But in chronic cases of disease, such as necrosis, gangrene, &c. amputation at the knee may be more advantageous.

M. Velpeau contends, however, that the operation at the knee is more easy, simple and less dangerous; and also that the stump will be more useful and convenient, and the deformity be more easily rectified by artificial means.

35. *Radical Cure of Hernia.*—In our last number p. 248, we presented our readers with a plan for a radical cure of hernia suggested by M. Belmas, and confirmed by many successful experiments on animals, and by one on the human subject. At the meeting of the Royal Academy of Paris on the 20th of December 1830 (Journal Hebdomadaire No. XIV), M. Breschet presented a report on two memoirs of M. Ravin on the same subject, in which a perfectly safe and simple mode is proposed, and which, M. Ravin declares, has actually succeeded in eight cases. The author believes that if no mechanical obstacle be opposed, the aponeurotic tissues will always slowly coalesce (revenir sur euxmemes), and obliterate the openings between their fibres. He hence proposes to keep the intestine as long as possible at a distance from the ring, and for this purpose, keeps the patient on his back with the pelvis more elevated than the rest of the trunk, and the affected side more than the sound, while astringent lotions, &c. are applied to the groin.

36. *Excision of the Elbow Joint.*—Mr Syme continues his Quarterly Reports of the Edinburgh Surgical Hospital, in the Edinburgh Journal; in the number for October last, we have the report ending the 8th of August 1830. He still recommends the practice of excision as being often preferable to amputation, and states that in ten cases he has lost but one patient. This was a man whose general health was undermined, and whose elbow joint was in a diseased and carious condition, with fistulous openings, &c. The operation was nevertheless performed on the 25th of June, and the patient somewhat improved. On the 15th of July, a large abscess appeared on his hip, which opened and proved to be an extensive lumbar abscess. On the 31st of July he died from the effects of the combined irritation.

Mr S. has since operated successfully on David Forret, aged twenty-eight years, who had been ill with a diseased elbow for nine months. He also reports that one of his former patients, Elizabeth Falkirk, whose elbow joint he had extirpated in the summer of 1829, and who had returned home and remained perfectly well, using her arm for all ordinary purposes, until December last, had returned with a diseased wrist of the same arm. Amputation above the elbow was now performed, and the patient recovered. The elbow being dissected, afforded a specimen of the union which is established in such cases. When the integuments and muscles were dissected, the appearance greatly resembled a natural joint, owing to a large mass of fibrous ligamentous looking substances which connected the bones together. This connecting medium, which was above half an inch in length, and perfectly flexible, resembled the structure that usually exists in false joints.

37. *Diseases of the Joints.*—Mr Syme has followed the advice of Larrey and of the French surgeons generally, in employing the actual cautery as a counter-irritant in many diseases of the joints. He relates several cases of inflammation of the various tissues of the hip-bone and elbow-joints, in which it seemed peculiarly efficacious.

38. *Excision of the Elbow-joint.*—In the Edinburgh Medical and Surgical Journal for January 1831, Dr James Simson relates a case in which he successfully extirpated the elbow-joint, for disease of the bones, &c. His patient, Mrs Rodgers, was forty-six years of age; the disease commenced in March 1829, and in November fistulous sinuses existed, and the general health was seriously impaired. The operation was performed on the 25th of November 1829; the greater part of the wound healed by the first intention, and in three weeks she could move her fingers without any pain, which she had not done for nine months before. By the end of December the wound was entirely whole, and she was walking about and moving slightly the joint without any pain. Her general health became much impaired, and a succession of abscesses formed in the sore arm for nearly three months. At the end of March she improved in every respect, and has since continued very well. She can bring her hand to her mouth, and is daily in the habit of using her arm in all domestic duties. The strength of the arm continues to improve. The connection of the radius to the humerus does not appear so firm as that of the ulna. October 30th, 1830.

39. *Ligature successfully applied to the Subclavian Artery, by Valentine Mott, M.D. of New York.*—The following case was reported in the American Journal of Medical Sciences, for February 1831, at page 309.

"William Hines, aged twenty-eight, of Smithville, Virginia, came to New York, August 24th, 1830, and became my patient.

"The account he gave of his case was, 'that about seven weeks ago he received a violent strain while carrying a canoe on hand-bars across the arms, which was followed by an extensive discoloration of the skin of the right arm, extending to the chest, and attended with considerable pain. It, however, yielded to the usual remedies in such cases. Three weeks subsequent to the accident he observed a swelling about the size of a pigeon's egg under the right arm, which had rapidly increased.'

"On examination, I found a tumour about the size of a goose egg, and decidedly an aneurism of the axillary artery. His general health being good, I directed him to keep quiet, to be bled, and to take some purgative medicines; and fixed on Monday, the 30th, for tying the subclavian artery.

"At 11 o'clock, A.M. he was placed upon the table, with the shoulders elevated and inclined to the right side. An oblique incision was made, two inches in length, through the integuments and platisma-myoides muscle, and corresponding to a middle line of the triangular interval formed on the inner side by the scalenus muscle, on the outer by the omo-hyoideus, and below by the cla-

vicle. The cervical fascia was next divided to the extent of an inch, and with the forefinger and the handle of a knife, the adipose and cellular tissues were put aside, and the artery readily exposed as it passes from between the scaleni muscles. After denuding the artery a little of the filamentous tissue with a knife rounded at the point and cutting only at the extremity, a ligature was conveyed around it, from below upward, by the *American needle*, and the artery tied a little without the scalenic muscles.

"No other ligature was required. The patient lost less than two tea-spoonfuls of blood. The operation lasted about fifteen minutes, and was performed, with the assistance of Drs Vache and Hosack, in the presence of Drs Barrow, Kissam, Rogers, and Wilkes. The wound was closed by two stitches and adhesive straps; the arm was immediately wrapped in cotton wadding; no diminution of temperature took place.

"8 P.M. Found the patient comfortable; says he has less pain in the arm than before the operation; heat rather more than natural; a faint pulsation in the right radial artery; pulse 88.

"31*st*, Morning. Passed a comfortable night after taking fifteen drops of the sol. sulph. morphine, which was given to allay the pain about the elbow, and which he considered rheumatic, having had more or less of it for some time previous to the operation. This pain was no doubt caused by the pressure of the tumour upon the brachial plexus. Pulse 70; skin natural; says that he feels very comfortable.—Evening. Complains of headach; directed a saline cathartic; pulse 90; skin pleasantly moist; pulsation in the right radial artery occasionally very distinct and regular; temperature of the right arm a little higher than that of the left.

"*September* 1*st*. Pain of the arm obliged him to sit up most of the night in an easy chair—after the operation of the salts, took again fifteen drops of the morphine, and slept quietly about five hours. Feels at present very comfortable; pulse 75; not the least evidence of febrile disturbance in any of his symptoms.

"2*d*. Feels much more comfortable than yesterday; slept composedly all night; little or no pain in the arm; pulse 80; removed the wadding from the arm, and enveloped it in flannel, which keeps it very comfortable.

"3*d*. Slept well all night after taking his dose of morphine, and feels very well to day; pulse 74; pulsation of the right radial more regular and distinct.

"4*th* and 5*th*. Continues to improve.

"6*th* and 7*th*. Every way comfortable; right radial pulsates regularly, though more feeble than the left.

"9*th*. Dressed the wound and removed the stitches; mostly healed, except where the ligature from the artery passes out. Pain in the arm for some days past has not been felt; makes no complaint; pulse in the radial artery very distinct and regular with the actions of the heart.

"11*th*. Dressed the wound, which looks remarkably well; every thing appears very favourable.

"14*th*. On removing the dressings to day, the ligature came away; all promises well.

"20*th*. Wound being just closed, permitted him to walk about the room, and to take his usual allowance of food; aneurismal tumour much diminished in size, and very hard.

"27*th*. Left the city to day on his return by water to Virginia.

"When I reflect on the disease for which this operation was performed, and upon the situation, importance and size of the vessel which was tied for its removal; it appears to me almost incredible that but twenty-seven days should have been required for its cure. That it should have succeeded is particularly grateful to my feelings, inasmuch as it was first successfully performed by an American surgeon*, and is an additional proof of the triumph of surgery over disease and death."

* Dr Post, of New York.

40. *Longitudinal Section of the Lower Jaw.*—Amputation of the lower jaw has now been frequently and successfully performed: but even when most successful, much deformity and inconvenience remain. To obviate this, Dr J. Rhea Barton of this city, so well known for his ingenuity and dexterity in devising and executing surgical operations, has successfully adopted in one case another mode of operating, which no doubt may frequently be imitated with great advantage. We shall give the details of the case from the Amer. Journ. of Med. Sciences for Feb. 1831.

"On the 28th of July, 1830, Isaac Duffield, a mulatto man, thirty years of age, from the State of Delaware, was admitted into the Pennsylvania Hospital, with a disease said to have originated in the gum which had been injured by the pressure of the fulcrum of the key, used in the extraction of a molar tooth of the left side of the lower jaw, eighteen months previously.

"It assumed the form of a tumour, involving the gums, alveolar processes, and part of the body of the bone, and gradually increased in size until it had taken entire possession of the mouth, forcing the tongue into the pharynx, and stretching the jaws widely apart. A large portion of the tumour also made room for itself by escaping from the cavity of the mouth, and rising upon the outside of the superior maxillary bone, thus producing great deformity by protruding the lips and left side of the face and neck. Where this loathsome mass presented itself at the mouth it was in a state of ulceration.

"The tumour was of a florid red colour. It was dense, elastic, and lobulated, and in its structure partaking much of the character of the gums. From its surface was secreted large quantities of muco-purulent matter.

"Upon introducing a spatula into the mouth, and putting the right cheek upon the stretch, the patient could force the tip of the tongue into view between the half arches and the side of the tumour.

"As he had neither room for solid food, nor power to masticate it, his nutriment was taken in fluid form by a similar process of distending the cheek.

"*Epulis* is the name assigned by writers to this species of tumour, which, though not malignant in its early stage, nor *essentially* so at any period, is said frequently to eventuate in a cancerous disease.

"The peculiarity of the operation I am about to describe consists in the *longitudinal* section of the bone, by which *a rim at the basis of the bone* was preserved to support the under lip and chin, thereby obviating the distortion of the face consequent to the usual mode of cutting out an entire piece of the bone by the perpendicular section.

"*Operation.* On the 4th of September, an incision was commenced over the left angle of the lower jaw, carried on a line with the under edge of the base, around to near the facial artery of the opposite side. This incision extended through the integuments and muscles of the cheek and lip, so as to open the cavity of the mouth. The under lip, towards the left commissure of the mouth, was cut through to meet the first incision at a right angle. The tumour was thus brought into view, exposing fully its connexion with the lower jaw, to the anterior and posterior surfaces of which it firmly adhered. The adhesions were detached from the anterior surface of the bone, as high from the base as it was sound. A section was then made with a saw, through the body of the bone, just below the maxillary canal, in a line parallel to the base of the jaw, and extended as far back as the roots of the last molar tooth, on the left side, and to the last but one on the right side. A perpendicular section was then made with a narrow bladed saw, between the teeth through the alveolar process, until it met at right angles the first division of the bone. The portion thus insulated contained the diseased mass. The tumour was then elevated, and its connexions with the inner surface of the bone separated. It now came away entire, exposing the body of the bone in a healthy state, except at one point on the surface, which was readily taken off with the nippers.

"No blood-vessels were taken up but the left facial and right coronary arteries; the hæmorrhagy from other points ceasing spontaneously. The flap was now

replaced, and the sides of the incision carefully adjusted; the lip closed with the hare-lip pins, and the rest supported by interrupted sutures and adhesive plaster.

"The patient did well; the wound united by the first intention, and in a short time the parts within the mouth healed over the bone. The tongue regained its natural position. The distended and flaccid lips and cheek gradually contracted to their natural dimensions, but their collapse was prevented by the rim of the jaw which had been left. The contour of the face was thus preserved, and the patient recovered the power of masticating his food with the three remaining teeth, and their antagonists of the upper jaw.

"On the 9th of October following, the man was discharged from the Hospital cured, and almost incredibly free from deformity."

41. *Varix.*—Mr Liston, in his late book on the elements of Surgery, says the Edinburgh Medical and Surgical Journal, page 200, for January 1831, is decidedly partial to the method of Mr Mayo in the treatment of varix, by inducing the adhesive inflammation by means of the caustic potass. He allows, however, that the practice is not free from danger, either from ulceration of the venous coats, and alarming hemorrhage, or from excessive spreading phlebitis. He recommends, therefore, that the caustic be always applied to the venous trunk at a healthy point.

42. *Dressing of Wounds.*—Mr Liston has in a great measure abandoned sutures, and also the resinous plasters, in dressing recent wounds. He uses instead of the resinous plaster, slips of glazed ribbon, immersed in a saturated solution of isinglas in brandy, which he represents as much less irritating and more tenacious than the common adhesive plaster. Sutures are employed to fix and retain the parts temporarily in opposition, and the ribbons are not applied till the adhesive substance has partly coagulated and the oozing of blood has ceased. After the application of the ribbon slips the sutures are removed.

43. *Ununited Fracture.*—We copy verbatim the following case from the New York Medical Journal for February 1831, as exhibiting one of the rare instances of recovery after the operation of excision of the ends of the fragments in an ununited fracture, even when the operation was not performed in the most delicate manner; a criticism very seldom applicable to the New York surgeon, whose celebrity as an operator is so well established.

"James Norton, a middle aged man, of good constitution, admitted April 5th, 1830. Ununited fracture of the os brachii. It was broken in the middle of January. Splints were applied until the time of his admission. About the 10th of April, Dr Mott passed a seton between the fractured ends, and Kent's apparatus for fractures of the arm was applied, by means of which the arm was kept at rest in a flexed position. Sept. 28th. No union having taken place, Dr Mott withdrew the seton a few days ago. To-day he made an incision on the external part of the arm down to the upper portion of the bone, the extremity of which he detached from the surrounding parts; he then passed a chain saw under it and sawed it off. The lower portion of the bone, which lay on the inside, was then laid bare with considerable difficulty, owing to its depth and to the vicinity of important parts, and the chain saw was passed under it; but in attempting to saw it off, the saw was broken. The extremity of the bone was then partly sawed off by means of a rotatory saw, and of Hey's saw, and partly broken off with bone nippers. It was then raised up, and sawed smoothly off with a metacarpal saw. The whole length of the bone removed was more than two inches. The extremities of the bone were then perforated with a drill, and united by means of well-annealed iron wire, the ends of which were passed through a double canula, and secured without. The edges of the wound were brought together with sutures and adhesive straps, and Kent's apparatus again applied. Two or three weeks after the operation, the wire came away, the arm became gradually stronger, and on the 15th of December, when he was dis-

charged, the union was complete. He had not yet, however, regained the full use of his arm, as the muscles were very much relaxed by the shortening of the limb."

44. *Lithontripty.*—This operation we have frequently alluded to in our preceding numbers, and have expressed favourable opinions respecting it, when judiciously recommended, and skilfully performed, by an experienced operator. Few cases are on record of its successful employment, excepting among those who have devoted most of their time and labour to this subject exclusively. The following case, from the New York Medical Journal for February, in which lithontripty was successfully performed by Dr L. Depeyre, adds to the list of exceptions, and constitutes, as far as we know, the first effectual attempt in this country. We shall therefore give the details.

"Mr B., aged sixty-one years, of a lymphatico sanguine experiment, remarkably corpulent, consulted me some time in the Month of October 1830. The patient had many symptoms which led him to suspect the existence of a stone in the bladder; the most convincing proof with him was, that for many years previously he had frequently passed many urinary calculi. Some of these had been sufficiently large to remain some days in the urethra, but he managed to disengage them without having recourse to surgical aid. Within a few months he had observed, that while passing his urine, the jet would be suddenly interrupted. The bladder certainly contained more urine, for the desire to pass more still continued. It was evident to me that there existed a calculus in the bladder, and a silver sound, which I introduced immediately, justified the prognosis.

"The patient, more anxious for the future than actually suffering from the disease, was naturally desirous of being released from his complaint. Lithotomy was not to be had recourse to, as the patient declared he would rather die than submit to that operation.

"I proposed lithontripty, and it was readily acceded to. In order to perform this operation, I could only obtain an instrument with a handle (*litontriteur a manivelle*), which is very imperfect and feeble in its operation; but as the calculus was probably of no great size, I supposed that this instrument would suffice.

"The bladder having been previously injected with tepid water, I introduced the instrument, and soon seized the calculus, the size of which I estimated to be about fourteen lines in its greatest diameter. As soon as the drill touched it, it appeared to be hard, and gave out a sound sharp enough to be heard distinctly by the patient himself, and by those who were present during the operation. As I soon discovered that the drill acted very slowly upon the stone, I suspended the operation, and resumed it a few days afterwards, with a new drill, which, however, did not wear away the calculus much faster than the first. After each of these operations, I was encouraged by observing that the patient passed considerable quantities of fragments of stone.

"The incomplete success of these two attempts induced me to cause to be made an apparatus like that of M. Civiale, and on the 29th of November I again introduced the instrument.

"The calculus was seized two different times, and was perforated through its whole substance on both occasions with a drill, three lines in diameter. The operation lasted fifteen minutes, and did not occasion the least pain. Upon withdrawing the instrument, the patient passed with his urine a whitish powder, mixed with fragments of different sizes, among which I noticed one about the size of a pea. The patient felt, during the remainder of the day, some inconvenience from being frequently compelled to pass his water, and each time it was accompanied with small fragments of the calculus. Emollient enemas and mucilaginous drinks so rapidly subdued this irritation, that the patient wished the operation to be repeated a few days afterwards. As a matter of precaution, I did not, however, resume it until the 8th of December.

"On this occasion I experienced much difficulty in endeavouring to seize the

calculus. It escaped twice just as I was about to make it secure. I finally succeeded in seizing it, and from the extent of the dilatation of the arms of the instrument, I was satisfied that I had it in an entirely new position. By cautious management, I was enabled to drill it throughout in three different places, and when I had finished the last drill, the dilatation of the arms of my instrument indicated a diameter of five lines. The whole time consumed in this operation lasted forty minutes. As in the preceding essays, the patient was merely fatigued from his position, and experienced no pain. When the instrument was withdrawn, he passed his urine, with an abundant discharge of fragments, some of which were as large as those which I have already noticed.

"During this operation I collected but a small quantity of the debris of the calculus, and only some of the larger fragments, and notwithstanding the gradual diminution of the arms of the instrument, I thought that fragments of the stone might still remain. The symptoms of the disease had never been violent, the desire to pass water continued, and although the urine flowed with an uninterrupted stream, yet my impressions were strong that the stone had not entirely disappeared. To ascertain this, I made three explorations in the course of fifteen days, with the arms of the instrument widely extended, and continued my researches with the greatest minuteness, without finding the least trace of any foreign body whatever. In the course of a few days I had the satisfaction of seeing the health of my patient perfectly restored; and he assures me that he feels nothing of his former disease."

VII. MIDWIFERY.

45. ***Prolapsus uteri.***—With the following very interesting cases, we have been favoured by Dr John Warrington of this city.

The great influence which the uterus exercises upon the female economy—the secret but powerful effect which certain derangements of its functions induce in parts of the system apparently remote, the extreme suffering which some displacements of this organ produce—sometimes suddenly and at other times gradually—the fineness of feeling which causes the patient to shrink from revealing her situation (if indeed she be aware of what it really is, which I believe is frequently not the case), even to him whom she believes to be the minister of health—should induce the practitioner to make himself acquainted with the various conditions and derangements of this central organism of the female, which are so destructive of her comfort.

There are strong reasons for believing that from derangements of locality derangements of function frequently ensue; to the former, the aged and the youthful, the married and the virgin female, are subject from various causes, and in different degrees. These, though produced suddenly or more slowly, are susceptible of much, and, in many instances, of entire relief, by the careful attention of the practitioner to the nature of the case, and the skilful adaptation of such means as circumstances shall indicate.

From my note book I have collected the following cases, which are given in considerable detail, to show how various may be the symptoms arising from such conditions of this important organ.

1829, 9th Mo.—I was requested to visit S. H. in haste, with information from the messenger that "she had an attack of gout in the hip." She was of a large muscular frame, latterly her health not good,—upon returning from a ride in the suburbs of the city, she stepped quickly out of the carriage on the pavement, and felt something suddenly give way at the lower part of the abdomen, and when she arrived in her chamber the distress became very severe; she felt a great bearing down, an unceasing desire to pass her urine, a drawing from the umbilicus which caused her to flex herself as much as possible, and apply warm moist cloths under an impression that it was an attack of colic. An acute pain was also felt

over the left hip bone, from the ramus of the pubis to a little posterior to the acetabulum. The stomach sympathised with the affected part, violent retchings ensued,—she was very restless,—her skin was covered with a profuse perspiration, while it was cold to the touch; pressure on the umbilical region afforded no relief, neither was the pain in the hip increased by pressure; there was no acceleration of the pulse.

I was led to suspect that there was a displacement of the uterus, and I suggested the propriety of an examination to ascertain the fact. The patient being placed in a suitable position, the os uteri was felt to be within an inch and a half of the os externum. I carefully replaced the organ in its natural situation, and directed the patient to remain in bed with the hips a little elevated. An anodyne enema to be administered, as several doses of laudanum given by the mouth had been rejected during the violence of the pain. Next day was passed comfortably; slight febrile reaction had taken place; this was removed by a gentle laxative; on the third day she was permitted to rise and walk a little; no unpleasant symptom occurring, she in a short time resumed her former habits of activity, and has continued well.

1829, 11th Mo.—E. W. aged thirty-two. A stout woman, accustomed to active exercise in housewifery, was seized with violent vomiting, and soon after with discharges of blood, reported by the patient to be from the bowels. By the use of anodynes and mucilaginous drinks these were soon checked, and she was treated for a time as suffering from wandering rheumatism, without, however, any permanent relief, and she at length assented to her friends' directing my attention to the most prominent seat of distress. She then stated that she suffered much pain in the region of the uterus; when she arose and stood erect, she felt as though something would fall away from her, and that she sometimes could not retain her urine, and at others could not evacuate it without some pain and much difficulty. A strange sensation was also felt down the thighs; she has also at times experienced a painful tumefaction of the left breast, which I had considered to be symptomatic of some morbid condition of the genital organs. She has been engaged in hard service, and about six months previously, whilst lifting a heavy weight, felt something suddenly give way in the hypogastric region, and a sensation of something escaping from the vagina. By a little rest, this was soon relieved. In her efforts at vomiting the same symptoms recurred, and have been her worst complaint since. This statement led to the conclusion that the uterus was dislocated; she reluctantly consented to an examination per vaginam. The os tincæ could not be felt; behind the neck of the bladder above the pubes, was a tumour of firm texture, laying nearly at right angles with the superior strait, and extending backward against the rectum. My impressions then were that the uterus was in a state of retroversion; that the pressure of the os tincæ upon the neck of the bladder kept up the constant disposition to expel the urine, and acted at the same time as the obstructing cause to this evacuation. The catheter was introduced with considerable difficulty and with much pain to the patient; a large quantity of urine was drawn off. Opiates were administered to procure rest through the night; the same difficulty recurring next day, a warm semicupium was directed to be used whenever the bladder became distended; this had a slightly temporizing effect. In three days the examination of the condition of the parts was repeated; first in the erect position, to ascertain the effect of gravity upon the tumour; in doing this, a resistance was offered to the finger soon after its extremity had passed between the labia; the same condition of parts presented as before. When placed in the horizontal posture, the tumour receded from pressure. The patient was now directed to go upon the breast and knees (the urine having been drawn off with a catheter), the hand was introduced in a wedge-shape into the vagina, and pressure made upon the fundus of the uterus, carrying it backward and upwards, till it receded beyond the reach of the fingers. The patient immediately expressed herself considerably relieved. To correct the fetid, and, at times, copious discharge, as well as to relieve the soreness of which she frequently complained, she was directed to use a watery solution of opium with sulphate of zinc several times a day as an injection

into the vagina. Under this treatment she improved rapidly for about a week, when by violent and imprudent exertion, the whole mischief was renewed, and somewhat aggravated. Then followed frequent, quick, and small pulse; great pain and extreme tenderness in the abdomen; difficult urination, attended with great heat; short, hurried, and laborious respiration. Eighty leeches were applied over the abdomen. The camphorated tincture of opium and sweet spirits of nitre were given in small and frequently repeated doses till the system was composed. Next day she felt better whilst remaining in bed, but, upon rising, felt a return of the pain, and had a discharge of purulent matter from the vagina. Directed her to wash the parts with soap and water, and have the bowels opened by an enema. Having procured a pessary of French manufacture under the name of gum elastic, I placed it under the uterus to support it. This procured her so much relief, that she again used great exertion; in two days she felt a return of pain, and when making an effort to evacuate the bowels, carried the instrument so far forward as to obstruct the urinary discharge. This was relieved by the catheter and the replacement of the instrument; after which, by means of strongly astringent injections, it was retained. About three weeks after, in consequence of a fetid discharge from the vagina, I removed the pessary; the abdomen is yet tender, and cannot easily bear pressure, but so much relief has it afforded her, that she is unwilling to dispense with its use. After cleansing it, I returned it to its place. In about a week subsequently I found her at her washtub, obviously improved in health, and quite able to partake of active exercise.

I have found it necessary to remove the instrument from its situation once in two or three weeks to have it well washed; the discharge becomes very offensive, and erodes the coating of the instrument, the roughened surfaces of which cause great irritation. The circumstances of the patient requiring public aid, she went into the alms-house during the winter. Whilst there another pessary of the same kind was used, the surface of which became quickly so much eroded as to require it to be removed, soon after which she relapsed into her former state of suffering. In the fourth month following, she again came under my care, and I substituted one made of glass, and to correct the fetid discharges which attended this situation of the uterus, I directed her to use a solution of chloride of lime in water. This had a happy effect. Since that time she has been much more comfortable, and during a period of seven months, there has been no occasion to remove the instrument from the vagina, though at one period, after a heavy fall, it became necessary to adjust its position a little. She is now able to perform nearly all the offices of a domestic, with a fidelity which endears her to the family of my friend with whom she lives.

1830, 2d Mo. 5th.—A. P. aged sixteen years, being one day at a distance from home, in descending a stair way, slipped and fell with her whole weight upon the sacrum against the edge of the step. When she arose, she found herself unable to stand erect without severe pain in the pubic region, and in her attempts to walk home, she was obliged to stop to rest, and seek relief by inclining forward so far as to relax the abdominal muscles, at the same time feeling a constant inclination to propel something from the vagina. I was requested to visit her on the 6th. I found her with fever, and complaining only of pain in the head and back. For this she was directed to lose ℥x. of blood, and have the bowels opened by sulph. magnes. In the afternoon of next day I found her complaining of pain in the back and hypogastric region. She had attempted to sit in her chair to rest from the fatigue of lying, and from her position and apparent suffering (supporting herself by her elbows upon the arms of the chair), I strongly suspected distended and inflamed hemorrhoids. Upon inquiry respecting the duration of these symptoms, I received the history of the accident, and found there had since been a constant bearing down, with inclination to pass urine and evacuate the bowels; and when standing, a dread of "something falling away from the stomach." I suggested my apprehensions to the mother, and proposed an examination per vaginam, to ascertain if the uterus had not suffered from the fall. This was consented to, and the os tincæ was found nearly in contact with the orifice of the urethra. At the posterior part of the vagina a tumour was felt,

which, on introducing the finger into the rectum, proved by its rotundity to be the fundus uteri, indicating a retroverted state of this organ.

The patient being directed to go upon the breast and knees, the reduction of the part was effected by the introduction of the fore finger of the left hand into the vagina, and that of the right into the rectum; pressing upward with the latter, and backward with the former, the fundus was passed upward and returned to its natural situation. So much was the patient relieved by this operation, that although she was directed to remain quietly in bed, she very soon afterwards arose, and walked about the room with entire freedom from pain, and the next day I had the satisfaction to find her at her usual avocation. She has, since the 20th, been able to walk a distance of several squares daily.

4th Mo. 17th.—E. M. G. aged thirty-five, widow, with several children. When pregnant with her last, about four years since, fell upon the ice, and suffered much from violent pain in the back. After parturition she became subject to leucorrhea and "great bearing down in the back and navel;" her health gave way, and she became emaciated, with constant fever; the hectic flush made its appearance upon her cheek; I suspected her to be labouring under the effects of a displaced womb; proposed an examination, which was reluctantly consented to, as she "could not believe that was her situation." The uterus was found low in the vagina, with its mouth projecting towards the left side of the pelvis. By steady pressure it was passed back as far as the finger would reach. A circular glass pessary was placed under it, and the patient requested to remain in bed a few days till the parts became accommodated to the new tenant. In a short time all the unpleasant symptoms were relieved, and the countenance wore a more agreeable aspect. To the back the emplast. roborans was applied, and to remove constipation she was directed to take the Pil. Rhei occasionally. At my subsequent visits I found her more comfortable. She menstruated soon after, but was again attacked with a distressing leucorrhœa, though to a much less amount than before. For this she was directed to wash the parts with soap and water, and use an injection of sulph. zinc. in the vagina. 8th Mo. 18th. Patient now enjoys good health, and is able to use active exertion.

4th Mo. 1830.—E. M. C. aged about thirty-five, mother of five children. About nine years since, soon after her marriage, she became so delicate as to be unable to walk, having a distressing weight and bearing down in the pubic region. Her pregnancies were attended with much distress, her last one particularly so, and what is remarkable, she has felt little or no amelioration after the period of "quickening." She is at present in the fifth month of gestation, and complains most of the time of pain in the lumbar region, with sometimes a dragging sensation about the umbilicus; for this she was twice cupped, with merely temporary relief. She does not refer to any pain in either side, or numbness in the lower extremities, neither is she much relieved by the horizontal position, but a constant uneasiness is felt in the lower part of the back. To get relief, she throws herself on the bed, then rises unrefreshed; thus, as she states, she spends much of her time. Her habits of life are very regular, and she has no fever.

Strong suspicions arose that she was labouring under some displacement of the uterus. Examination gave proof of a relaxed state of the vagina, and a prolapsus of the womb, which was felt to be quite low. With gentle, but firm pressure, it was restored to its proper place. A glass pessary was introduced, and the patient directed to remain forty-eight hours in bed to give the parts time to accommodate themselves to the instrument. On the third day she resumed the charge of her family, and was found actively employed in her domestic concerns, declaring herself to be greatly relieved. She continued well till one day about three weeks subsequently walking a distance of twenty-two squares, she returned fatigued, with some pain in the back and pubic region; in the evening the pessary was passed by an effort to relieve the bowels, and the pain formerly complained of returned. I was again requested to see her, and finding her very uncomfortable, I again placed the pessary under the uterus. She was immediately relieved. Calling to see her a few days subsequently, I found her closely employed in the duties of her station, quite comfortable. The patient continued to do well throughout the subsequent part of gestation. I removed the pessary about two weeks

previous to her confinement, which occurred on the 21st of 9th Month. Since that time she has not required my attention.

7th Mo. 5th.—E. A. aged fifty-one years. Mother of twelve children, countenance expresses great anxiety and distress; in standing or walking, inclines a little forward, cannot ascend stairs easily, complains of a "burning sensation at the lower part of the stomach," which she attributes to a "bowel complaint," with which she is often, indeed almost constantly annoyed, the evacuation of the urine is attended by heat. She is much troubled with piles, and is constantly threatened to have something fall from the vagina when erect. The patient is febrile and emaciated, the pulse is small and frequent, her disposition extremely irritable. I suspected some derangement of the uterine locality. Examination revealed a complete relaxed condition of the vagina, the os uteri just protruding between the labia which were of a spongy texture. The meatus urinarius was very tender to the touch. All these parts were in a highly sensitive condition. Pressure upon the womb caused it to recede and at first gave much pain—by placing the patient in a horizontal posture it was reduced without much difficulty. The hemorrhoidal tumours were large and very painful under the slightest pressure. The patient was directed to remain in bed, keep the parts clean by frequent ablutions with fine soap and water, and to use three or four times a day an injection of the following preparation.

R. gallæ allep. contus. one dr. infuse two hours in a pint of boiling water, strain and add sulph. zinc, one dr. This powerfully constringed the relaxed external parts, removed the prolapsed state of the vagina, which the patient informed me had been so considerable at times as to present itself beyond the labia, and to support it she had at times worn bandages, which increased the inconvenience by causing excoriation. She was now considered in a fit state for the pessary—the pressure made upon the neck of the bladder in passing it gave much pain, but when placed in its proper position, the patient was insensible of its presence.

Rest and the horizontal posture with the frequent use of the astringent injection were strictly attended to, for a few days. She then rose and walked with a facility which surprised her; she has urinated freely since and has none of that tenesmus which had heretofore distressed her very much. A laxative diet, with a little oleum ricini occasionally, have been necessary to keep the bowels open, and the hemorrhoids have been relieved by the application of Goulard's cerate.

7 Mo. 23d.—Walked 17 squares to report to me her entire relief from her former suffering,—can now walk with ease on a level surface, though she finds some difficulty in ascending stairs—her appetite is good and her health is much improved. To this condition of the uterus she had been subject during the last eleven years—and attributes it to a strangury caused by a large blister which had been placed over the abdomen a few days after the birth of her tenth child, and says her last pregnancy was more severe than those which had preceded it.

8 Mo. 23d.—I was requested to visit E. H. aged thirty-six; mother of several children, of active habits and usually enjoying good health; about nine months since, she felt an uneasy sensation in the pelvic region and a difficulty in passing urine. After an intermission of a few weeks, these symptoms returned with greater violence with weariness in the loins, a dull pain in the groins, and a burning pain in urinating. She became feeble, lost her appetite and applied to her physician for advice. By him she was directed to remain in the horizontal position, keep the bowels open by saline laxatives, and restrict herself to a vegetable diet, be bled from the arm and leeched upon the hypogastrium occasionally—all these directions except the last she had complied with, and had remained in bed about six weeks without any improvement.

When I saw her, she could not stand or walk without pain, and found it almost impracticable to ascend or descend stairs. There was no pain felt in either side; there was a sense of a weight depending from the umbilicus, and the whole abdominal viscera appeared almost to have subsided within the cavity of the pelvis. Examination per vaginam demonstrated a prolapsus of the uterus with enlargement of its mouth and neck, these were low in the pelvis, but directed backward into the posterior extremity of the vagina—the parts were restored to their proper

position, and a circular glass pessary was placed under them.—After using an astringent injection of a solution of sulphate of zinc and opium, she was able to walk with ease. Could ascend and descend stairs readily.

The above cases have not been presented to the reader in consequence of their rare occurrence, but to show that they are of greater frequency and of rather more importance than practitioners generally suppose. If hernia, with its occasionally terrific consequences, so much dreaded by the suffering it causes in our own sex, has interested the attention of men of the highest eminence in our profession, should not also the displacements, to which the uterus, from its peculiar situation and mode of attachment to the surrounding parts, is even more liable? Though perhaps less sensible and less intimately associated with the vitality of the system, than the tender bowels, yet the extreme suffering which sometimes attends the deranged condition of this organ, the languishing load felt at the back, the distress about the hypogastrium, the great deprivation which many females, particularly mothers feel, in being unable to use much exercise on foot, and the large, fetid, debilitating and frequently excoriating discharge from the parts concerned—are circumstances which demand, notwithstanding the passiveness with which the patient endures the malady, that it should be studied, and that where our skill can be interposed we should afford relief.

The reluctance on the part of the patient to reveal her situation often prevents the interposition of relief till extreme pain, or debility or some other distressing consequences have taken place. Indeed it is sometimes the case, that she is ignorant of the causes which induce these symptoms, uneasiness being frequently felt in a remote part, while the real situation of the disease is unsuspected.

Sudden though partial prolapsus appears to be productive of more suffering than one slowly though extensively formed. I lately had occasion to attend two females, who, from great exertion in lifting, were attacked with pain in the lower part of the abdomen and loins, frequent efforts to urinate, &c. in both of whom the restoration of the uterus to its proper situation caused immediate relief, and after confinement to the bed a few days, they have continued well—while another patient, of whom I have charge, has for more than five years past, been so subject to an entire procidentia uteri, that its surface is nearly like the skin of the abdomen itself, yet, notwithstanding she has had to perform active and laborious exercise, now in the 53d year of her age, and had felt it merely a slight addition to the infirmity of increased years, till during the extremely warm weather of last summer, when her health failed very much and she was obliged to solicit advice.

The whole uterus, somewhat enlarged was found depending at the os externum. It was returned with little difficulty as the perinæum had been extensively lacerated during a badly conducted parturition about twenty-four years previously. A very large pessary was introduced which supported it partially and affords much relief and she is again recruiting. Whether this comparative exemption from pain results from the fact, that at this time of life the uterus has ceased all its functions as a gland, I am not fully prepared to say, though I suspect this has some influence over its sensibility. Certain it is, that during the menstrual periods of the female whose uterus is displaced, her sufferings are mostly increased, the flux is irregular and the intervals are frequently filled up by a distressing leucorrhoea.

The treatment of these cases varies according to circumstances, but where there has been much pain and constipation I have had the bowels kept open by small and frequent doses of rhubarb and extract of henbane—this usually agrees well with the patients as a laxative in the following proportions

R. Pulv: Rhei 1 dr.
Ext. Hyosciam. 1 sc.
M. ft. Pil. xx.

One of these to be given every one, three, or six hours pro re nata.

In many cases local depletion is necessary, by cups or leeches to the sacrum or pubic region, in a few instances I have directed blisters to the sacrum, but in all these cases there were symptoms of local inflammation.

Occasionally I have had astringent injections used as a preliminary to the use of the pessary, but have frequently at once introduced this instrument. After some trial with those made of cork covered with wax, and those of the French manufacture resembling the gum elastic and finding them liable to several objections, I have had them made of glass, which I think will be found to possess the following advantages: 1st. No fluid secreted by the uterus or vagina can destroy the material.

2dly. The perfect smoothness of the surface, to which none of the secretions can firmly adhere—a few syringes full of tepid soap and water being quite sufficient to keep them clean without removing them from the vagina.

3dly. The little irritation they produce in the vagina, many patients not being sensible of their presence in that situation.

My friend William Scattergood, to whom the suggestion was made, has with much care succeeded in getting the article neatly manufactured, and from his shop at the south-west corner of Second and Green streets they can be obtained of various sizes.

46. *Stethoscopic signs of Pregnancy.*—In the Dublin Medical Transactions, Dr Ferguson has published an account of his success in detecting pregnancy by means of the stethoscope. He says he has never in a single instance been mistaken; and in one instance only of undoubted pregnancy, was unable to detect the stethoscopic signs. Two distinct sounds may be detected; one arising from the pulsations of the fœtal heart; the second from the placenta, resembling a feeble thrill, which Dr F. refers to the passage of blood in the arteries connecting the placenta to the uterus. The placentary noise may generally be found in one of the iliac regions, and in the same subject is always to be found in the same place. It may be mistaken for the pulsations of the iliac arteries, but this last will be heard perhaps in all cases on both sides, and can only be perceived in the groin, while the noise produced by the placenta is heard over some space, perhaps three or four inches square. The fœtal heart may often be heard in every region of the abdomen. Although it and the placenta are sometimes heard on the same side, and even in the same spot, yet usually they are on opposite sides; the fœtal heart is generally perceived in one of the iliac regions, but unlike the placenta, it is not always heard in the same place, in the same individual, but it does not vary much from the point where first heard. Its double beat is well marked; and the frequency of its pulsations is, says Dr F., always much greater, often double that of the mother's.

In the Dublin Hospital Reports, this subject is also treated of by Mr Kennedy, who agrees with Dr Ferguson that the placental thrill is owing to the transmission of blood from the arteries of the mother to the placenta. He appears, however, to consider this thrill as important in detecting pregnancy in its early stages before quickening, and before the pulsation of the heart can be recognized. He has noticed the placental thrill as early as the 10th week, and hence in many cases this diagnostic mark of pregnancy becomes extremely valuable,—cases to which particular reference is unnecessary.

VIII. CHEMISTRY.

47. *Analysis of the Pulmonary Perspiration.*—M. Collard de Martigny gives the following as its composition.

Water	907
Carbonic acid	90
Animal matter	3
	1000

M. Collard could detect no ammonia. Now if Lavoisier's idea were correct that for the production of the moisture, the hydrogen of the venous blood under-

goes a true combustion in the lungs, it must be assumed to be nascent hydrogen; but it has been proved, that wherever nascent hydrogen is in contact with nitrogen, as it would be, in this case, in the lungs, ammonia is formed. Therefore on Lavoisier's theory we ought to find this alkali; and as we do not find it, M. Collard thinks the view of Lavoisier falls to the ground.—*Journ. de Chim. Méd. No.* XXIII. *Dec.* 1830.

48. *Effects of the Respiration of different Gases.*—In our number for July last, vol. x, p. 217, we gave a note on the effects of oxygen when respired, according to the experiments of Mr Broughton. This experimenter tried the effect of a number of other gases. Nitrous oxide had precisely the same effect as oxygen, but acted with more rapidity; that is, it acted as a narcotic poison, producing a universal arterialized state of the blood, and exciting symptoms of severe nervous derangement. A kitten was killed in half an hour, sparrows in four or five minutes, mice in about eight minutes; but a rabbit, three weeks old, lived two hours and a quarter. In chlorine all the animals tried were killed in about thirty seconds. The lungs presented the yellow tint and peculiar odour of the gas, so that, contrary to what is usually thought, chlorine passes the epiglottis. Hydrogen proved fatal in half a minute, and immediately after death, the heart and intestines were found to have lost their contractibility; and hence Mr B. infers that it acts as a positive poison. Carburetted hydrogen [olefiant gas?] acted with rapidity, causing one or two gasps and stupor in a few seconds. The experiments on nitrogen, sulphuretted hydrogen, nitric oxide, and carbonic acid, present no particular novelty.—*Edinburgh Medical and Surgical Journal, for October* 1830.

49. *Mutual Action of Blood and Atmospheric Air.*—In consequence of the recent publication of a paper by Dr Davy, in which he denies the mutual action of atmospheric air and blood recently drawn, Dr Christison of Edinburgh undertook an experimental examination of the point; and his results are given in a paper, contained in the Edinburgh Medical and Surgical Journal for January 1831.

The usually received opinion is, that out of the body venous blood absorbs oxygen and gives rise to the formation of carbonic acid. Dr Davy denies this, and asserts, that the carbonic acid, when it appears, arises from incipient putrefaction, which generally takes place in the blood within twenty-four hours, the period during which the contact has been usually allowed to continue. Dr Christison's experiments, however, which appear to have been accurately made, confirm our original opinions on this subject, and seem to prove that Dr Davy's results are inaccurate.

Dr Davy contends that the change, produced by shaking venous blood with air, is more apparent than real, being caused by the bloody froth which is produced, and disappearing when this froth subsides. Dr Christison, on the other hand, has satisfied himself that the change is real and permanent. Dr C. has also found the assertion of Dr Davy inaccurate, that venous blood, when agitated with hydrogen, undergoes the same change as when agitated with atmospheric air.

Dr Christison next takes up the second point,—what are the changes produced on the air. He performed his experiments on venous blood, which was prepared by removing the fibrin without contact with the air. In every case oxygen disappeared, and carbonic acid was formed; but the quantity of the latter was not so great as that of the former; so that in all cases there was a sensible diminution in the gaseous contents of the bottle in which the agitation was made. The diminution is considered by Dr C. to arise from the absorption of carbonic acid by the serum, which fluid has been proved, both by himself and Dr Davy, to have a strong solvent power over carbonic acid.

It might be a question whether the carbonic acid was formed at the expense of the oxygen, or independently of it. Dr Christison considers it proved that it was

formed at the expense of the oxygen; for the amount of oxygen in the residual air, and in the carbonic acid, added to the total absorption, equalled the quantity of oxygen in the original air. It may not be perfectly correct to say with Dr Christison, that this explanation of the source of the carbonic acid is proved, though it is highly probable; for it may be conceived that a portion of oxygen is absorbed exactly equal in bulk to the carbonic acid formed.

Dr Christison is of opinion that the nitrogen remains unaltered in volume. The slight differences which he obtained in his experiments, he very properly attributes to unavoidable errors in analysis.

Dr Christison's view of the subject, from his experiments, is shortly this: air, by agitation with blood, loses oxygen, which is converted into an equal bulk of carbonic acid. Part of this carbonic acid appears as such, but the remainder, by being absorbed by the serum, is condensed, and causes the diminution in the gaseous matter observed in the experiments. As, says Dr C., blood and air out of the body undergo the same changes as in the lungs, therefore, these changes occurring in respiration are a chemical and not a vital process.

According to the probable explanations of Dr C., the amount of the carbonic acid produced, and of the absorption taken together, represents the total amount of oxygen expended. This varied in different experiments from 1.4 cubic inches to 0.57 of a cubic inch, in 10 cubic inches of blood. These differences in the expenditure of oxygen, Dr C. considered to be too considerable to have depended on mere differences in the degree of exposure of the blood to the air, in the act of flowing from a vein; and, therefore, enters into the question as to the probable causes of them. These causes he considers to be two:—1. The difference in the degree of venalization of the blood in passing through the capillaries in different cases; and 2. The difference in the proportion of colouring matter contained in the blood.

The explanation given by Dr C. in support of the reality of these causes are satisfactory. Every physician must have noticed with him the comparative venous state of different specimens of venous blood. Even in the same venesection, the blood which flows first will be often strongly venous, while in the progress of its flowing, it will approach to the arterial state. In some acute diseases, it will have an arterial character from the moment the vein is opened. Dr C. observed this to be the case particularly in acute rheumatism. Now it is plain, that the nearer blood approaches to the arterial character, the less oxygen will be expended to bring it fully to the arterial state.

As it is the colouring matter which is acted on by the air, it is obvious, that Dr C. is right in asserting that differences in its amount will give rise to differences in the quantity of oxygen consumed; and every one will admit that in certain diseases the colouring matter is deficient. Dr C. adduces the instance of the disease of dropsy.—*Edinb. Med. and Surg. Journ. Jan.* 1831.

50. *Ozmazome.*—M. Collard de Martigny undertook some experiments to determine whether the principle called by this name is formed wholly or in part by the heat and moisture, by means of which it has heretofore been obtained. To determine this point, he submitted fresh muscular fibre, free from fat, to the action of alcohol in the cold, and obtained a white fatty matter, and a brown matter having all the characters of the osmazome of Thouvenel and Thenard.

M. Collard subsequently fell upon a method of separating ozmazome from albumen. If the tincture of iodine be added to a concentrated solution of ozmazome, it produces a thick, yellow cloud; if to one of albumen, reddish-yellow flocks. These precipitates are considered to be iodides. If a mixed solution of the two principles, the two precipitates are thrown down together, they may be separated from each other by ammonia, which dissolves the iodide of albumen and leaves the iodide of ozmazome untouched.—*Journ. de Chim. Méd. No.* XXIII. *Dec.* 1830.

51. *New Method for obtaining Iodic Acid; by M. Serullas.*—M. Serullas, having satisfied himself that the perchloride of iodine becomes, by solution in

water, muriatic acid and iodic acid, and recollecting that the former acid is soluble in alcohol, and the latter perfectly insoluble, supposed that these facts might be made use of for obtaining iodic acid quite pure. His conjecture proved to be correct; for by repeatedly acting on moistened perchloride of iodine with alcohol, the muriatic acid was removed, and perfectly pure iodic acid left. In order to succeed well in the experiment, the perchloride employed must be carefully freed from subchloride.—*Journ. de Chim. Méd. No.* XXIII. *Dec.* 1830.

52. *New Test for Organic Alkalies.*—M. Serullas proposes as a test for these alkalies in alcoholic solution the use of a weak solution of iodic acid, or a solution of perchloride of iodine, which acts by reason of its containing this acid. It operates on the principle of forming insoluble supersalts with these bases, and its delicacy depends in part upon the large relative quantity in which it combines with each particular base, thus forming of itself nearly the whole of the precipitate. It is particularly sensible as a reagent for quinia and cinchonia, and hence it is that M. S. hopes to be able to apply it to the purpose of ascertaining the relative medicinal value of Peruvian barks.—*Journ. de Chim. Méd. No.* XXIII. *Dec.* 1830.

53. *Chloroxalic Acid.*—M. Dumas of Paris gives this name to a new compound, which he obtained by the action of dry chlorine in excess on crystallizable acetic acid, in the sun's rays. One portion of the chlorine combines with part of the hydrogen of the acetic acid, forming muriatic acid, and another portion, with the carbon, oxygen, and the rest of the hydrogen of the acid, forming the new compound.

Chloroxalic acid is solid and crystallizes in rhombs. It is extremely deliquescent. On distillation it yields muriatic and carbonic acids, and a carbonaceous residue is left. It consists of

Chlorine	36	1 atom,
Carbon	12	2 atoms,
Oxygen	24	3 atoms,
Hydrogen	1	1 atom.

This is equivalent to

One atom of chlorine,
One atom of dry oxalic acid, and
One atom of hydrogen.

The above is condensed from an article in the Journal de Chimie Médicale, No. XXI, for Nov. 1830. The quantity of oxygen in the compound is erroneously given as an atom and a half instead of three atoms.

MISCELLANEOUS.

AN ESSAY ON THE HISTORY, PREPARATION, AND THERAPEUTIC USES OF IODINE. BY SAMUEL J. HOBSON, M.D. *Member of the Philadelphia Medical Society. Philadelphia*, 1830. 8vo. pp. 60.

The heads of the three sections into which this essay is divided will serve to show its scope and purposes. The first comprises the *history* of iodine, including its *physical* and *chemical properties*, its *combinations*, &c. The second, the *mode of obtaining it* and *its compounds*. The third, its *therapeutic application*, including its *action upon the animal economy;* its *pathological effects*; its *medicinal properties;* its *modus operandi*. We can recommend to our readers this essay of Dr Hobson, which, by the way, obtained the premium offered by the medical society of the state of New York, as giving an account, with the references to the works and authors, of what has been done by physicians with iodine. He does not pretend to any higher merit than that of an industrious compiler. He tells us what others say they have succeeded in performing by the administration of this medicine, and to the large majority of medical readers the information will, we are persuaded, be both new and useful.

A Popular and Impartial estimate of the present value of Vaccination as a security against Small Pox, and of the danger of encouraging or tolerating the inoculation of the latter, addressed to parents and the public. By Samuel Plumbe, M.R.C.S. Senior Surgeon to the Royal Metropolitan Infirmary for Children, &c. London, 1830. 8vo. pp. 99.

To remove the doubts which have been excited in the minds of many parents in consequence of the late extensive prevalence of variolous epidemics, as to the perfect security against the fatal effects of small pox, enjoyed by those who have been subjected to vaccination, and to convince them of the real nature and value of the preservative powers afforded by the latter when successfully performed, are the objects which the author had in view in presenting the present treatise to the public; and we conceive that he has performed his task with very considerable ability. Although to the well informed practitioner the work will present nothing new, it nevertheless contains much useful matter, well adapted to enlighten the public mind on the all important subjects of which it treats, and with which it is essential that every parent should be made acquainted. To the accuracy of the author's statements, and the soundness of his arguments, we can very generally give our cheerful assent.

Medical and Surgical Memoirs, by Nathan Smith, M.D., late Professor of Surgery and of the Theory and Practice of Physic in Yale College. Edited with Addenda, by Nathan R. Smith, M.D., Professor of Surgery in the University of Maryland. Baltimore, 1831. 8vo. pp. 374.

The late Professor Nathan Smith of Connecticut was a man of no ordinary talents. He enjoyed considerable renown in the eastern states, and his opinions

on surgical subjects were held in great esteem in every part of the union. Such being the case, many physicians of this city eagerly put their names to a subscription paper, circulated among them some time ago, for an edition of his works. The volume lately made its appearance, and has been without delay examined; but we are sorry to say, it has sorely disappointed, not only ourselves, but almost every one with whom we have conversed on the subject. We do not mean that the essays by Dr Smith which it contains are inferior in point of value to what we expected, but only that they are far less in number compared with the size of the volume, than what we anticipated they would have been.

Thus, instead of finding the work, which is a goodly octavo of three hundred and seventy-four pages, to contain, as we fondly expected, and as is indeed announced in the preface, the posthumous works of the venerable professor, we discover that all that may be claimed as his consists of an essay on typhus fever, already long before the public, and now reprinted with a few additions and notes by the editor; with four other essays—on Necrosis, on Fracture of the Femur, on Suppression of Hemorrhages, on Amputation; the details of a case of Ovarian Dropsy; of one of Ligature of the External Iliac Artery, and lastly, of a description of a New Instrument for extracting foreign substances from the Œsophagus. The whole of these articles occupy but one hundred and sixty-six pages. The remainder of the volume is made up of twenty-nine pages of a biographical memoir, nearly the whole of which is taken from an eulogium on Professor Smith by his colleague Professor Knight of Yale College, and of one hundred and twenty-seven pages of essays and cases by the editor, in only one or two of which we find a reference to the opinions of his father. If to this it be added, that to the publications of the late Professor Smith, the editor has added notes and supplements amounting to ten or fifteen pages of matter, we shall find that the volume before us consists of about one hundred and fifty pages by the author, for whose work solely we subscribed, and of two hundred and twenty pages by other hands.

Had the intention of resorting to this mode of swelling the volume been candidly stated at the time subscriptions were collected for its publication, some abatement in the eagerness with which many placed their names on the list of purchasers might have been experienced; for however much we all respect the talents and character of Professor Smith of Baltimore, we doubt whether the time has come when proposals for the publication in a volume of his surgical essays would meet with much encouragement. In some twenty years hence, we or our children will, we doubt not, have sufficient cause to think otherwise.

Be this as it may, we must be allowed to remark, that the editor of the work before us has adopted a convenient mode for ensuring the publication of his essays in a form that commands more attention than that afforded by the usual vehicle in this country for articles of the kind—medical journals, and that he could not have bethought himself of a more secure way of occupying an honourable place in the book cases of his medical brethren, and of being handed down to posterity, than by being bound up in company with his respected and talented father.

A Rational Exposition of the Physical Signs of the Diseases of the Lungs and Pleura; illustrating their pathology, and facilitating their diagnosis. By Charles J. B. Williams, M.D. Reprint from the London edition by Carey and Lea, Philadelphia. 8vo. pp. 205.

In this treatise Dr Williams has attempted to explain the mechanism by which the physical signs attendant upon disease of the lungs and pleura are produced, and the manner in which, according to fixed laws, they result as phenomena; to make a knowledge of the pathology of these diseases predicate the signs, and a knowledge of the signs indicate the pathology, "and by thus familiarizing the mind with teir principles, to enable it to understand the multifarious forms, which, by combination, these signs may assume, and to judge of the corresponding physical changes that modify or produce them." The work is undoubtedly one of

very great merit, and, however we may differ from the author in regard to some of his views, we recommend it to the attentive perusal of the physician and student: they cannot fail to acquire from it much information in regard to the indications derived from the application of the stethoscope in thoracic diseases and to other important points connected with their accurate diagnosis.

Coloured Drawing of the Great Sympathetic Nerve. By J. P. Manec, D.M.P. Lecturer on Anatomy, &c. at Paris. Copied on stone by P. Ancora, teacher of drawing and painting, Philadelphia. Letter press, translated and corrected by J. Pancoast, M.D.—Of this splendid and very accurate plate, we have already expressed our opinion in a note on page 364, of the present number. It is recommended to the attention of every practitioner and student, to whose offices or rooms it will form at once an ornamental and very important addition.

An Oration delivered before the Philadelphia Medical Society, February 19, 1831. By Thomas Harris, M.D., Honorary Member of the Society, Surgeon of the United States Navy, Member of the American Philosophical Society, Lecturer on Operative Surgery, and one of the Surgeons of the Pennsylvania Hospital. Published by the Society.

The orator has made choice, for the subject of his address, of the influence of exercise in promoting the health and vigour of the human system, and has treated it with great taste and judgment. The oration will be read by every member of the profession with much pleasure, and we are persuaded they will derive from it many useful hints in regard to the effects of exercise upon the digestive, circulatory, nutritive and intellectual functions of the system; the peculiar circumstances under which one or other of the various modes of exercise is to be preferred, and those in which it becomes objectionable. These points are considered by the orator with a degree of distinctness which was scarcely to be expected, considering the narrow limits to which he was necessarily restricted.

The Medical Formulary: Being a collection of prescriptions derived from the writings and practice of the most eminent physicians in America and Europe, to which is added an appendix, containing the usual dietetic preparations, and antidotes for poisons. The whole accompanied with a few brief pharmaceutic and medical observations. By Benjamin Ellis. M.D. Professor of Materia Medica and Pharmacy in the Philadelphia College of Pharmacy. "*Morbos autem, non eloquentia remediis curari.*"—Cels. De Med. Lib. 1. Third edition, with additions. 8vo. pp. 231. Philadelphia, Carey and Lea.

The medical formulary of Dr Ellis has been for some time before the public, and so far as we have been able to learn, has met with very general approbation. Having examined with much care the present improved edition, we feel much pleasure in being able to recommend it to the attention of the medical profession throughout our country. Comprising a collection of the most approved formulæ for the exhibition of remedies, with occasional remarks in relation to their medical properties and the rules for their administration, the medical formulary will form a useful, if not indispensable, addition to the library of every young practitioner. Much useful matter will be found in its pages, which cannot be procured with the same facility from any other source, an acquaintance with which will be calculated to remove much of that embarrassment which every novice in the profession experiences when called upon to decide as to the best forms for the administration of his remedies. An attentive perusal of the formulæ collected by Dr Ellis will, it may be hoped, be in some degree the means of introducing a more correct and classical latinity, and a greater degree of scientific accuracy into the extemporaneous prescriptions of the physicians of this country than has heretofore been the case.

We regret, however, that more attention had not been paid to the typographical execution of the work, so as to have dispensed with the long list of errata which is appended to it. By the ordinary reader, very seldom any attention is paid to the correction of these previously to, or during the perusal of a work; —in two or three instances the errors are of some importance.

EUROPEAN POPULATION.

At a recent meeting of the Royal Academy of Medicine, at Paris, M. Moreau de Jonnès read a memoir, entitled "a statistical sketch of the division of the populations of Europe into different series according to the ages of the individuals who compose them." In this memoir the author examines successively the relation which each series of ages bears to the entire population of each country. From various observations he has found that, in reference to its population, Ireland is the country in which there exists the greatest number of infants or individuals under twenty years of age, they being as 1 is to 2, or composing one-half of the population, while in France they are only in the proportion of 1 to 3, and in Paris, especially, only as 1 to 4. Children under five years of age bear in Ireland a proportion to the population of 1 to 5; in London, of 1 to 7; in Paris, of 1 to 14; and in all France of 1 to 8. The proportion of individuals between twenty and thirty years of age, M. Moreau found to be very nearly the same in each country of Europe—forming one-sixth of the population. In Paris, they constitute one-fifth of the population. It is at this period of life that man is best enabled to resist the causes of destruction. Of persons between thirty and forty years, is composed one-seventh of the population of France and Sweden, and one-eighth of that of England and Scotland. One-eighth of the population of France, and one-ninth of that of England, consists of individuals between forty and fifty years of age. In France the proportion of persons between fifty and sixty is as 1 to 11; in England, as 1 to 15. France possesses a greater proportion of individuals between sixty and eighty than England, while the proportion of those between ninety and one hundred is the greatest in England. M. Moreau found that the number of those between sixty and seventy years was twice as great in Paris as in London, and that of individuals between ninety and one hundred, the proportion was six times greater in London than in Paris. M. Moreau therefore concludes that the active population, or that composed of individuals between fifteen and sixty years of age, constitutes in France two-thirds of its inhabitants, while in every other European state it forms but one-half. Hence it results that France, from an equal population, is able to bring into the field more soldiers by one-eighth than England.

According to the last census, we find that in the city and county of Philadelphia, those under twenty years of age bear to the entire population the proportion of 1 to 2; in the city proper, of 1 to 2.4. Those under five years, in the former, a proportion of 1 to 7; in the latter, of 1 to 8.8. Those between twenty and thirty years, a proportion of 1 to 5 in the whole county; in the city, only, 1 to 4.9. Those between thirty and forty years, a proportion in the first of 1 to 7.5; in the last, of 1 to 8.4. Those between forty and fifty in the entire county, are in the proportion of 1 to 13; in the city alone, in the proportion of 1 to 14.4. Persons between fifty and sixty years bear a proportion to the population of the whole county, of 1 to 24; of the city merely, of 1 to 25. In the county, the individuals between sixty and seventy, are as 1 to 43.6; in the city, as 1 to 46. To every 346 individuals of the whole county, and every 319 of the city, there is one between 80 and 100. These calculations have reference only to the white population of Philadelphia.

We learn from an Edinburgh periodical, that at a recent meeting of the Royal Society of that city, a curious paper by Dr Duncan was read on the injection of air into the cavity of the chest in certain diseases affecting its viscera. A case was related. The patient had suffered much from an affection of the heart, and he

was led to think that the pressure of a small volume of air internally might be substituted for an external pressure which afforded him relief. The patient became his own operator; he employed a fine silver tube about as slender as a common pin, to which a bladder was attached, containing atmospheric air. The point of this was thrust through the skin and other integuments, till it reached the cavity of the chest, and the air was then forced through it by compressing the bladder. Relief was always experienced at the time, and a lasting improvement was effected in the patient's health. Very full details were given of the experiments, which were continued through the space of two or three years. Dr Lizars stated that he had performed the same operation upon four or five patients in cases of aneurism, always with some immediate benefit, and in no case with any ultimate injury. The apparatus with which the first patient operated was shown, and all doubts as to the perforation of the tube were removed, by blowing air through it into water. The experiment is curious in a physiological point of view, for if air can be conveyed into the chest in this manner, other elastic and inelastic fluids might be injected in the same way. And may it not be possible to extract fluids from the interior of the body by reversing the process?

We give the whole of the above just as we have found it in the foreign journal, without comment.

PHILADELPHIA MEDICAL SOCIETY.

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In the review of Dr Gross's work on the Bones and Joints, contained in our last number, page 114, it is stated, "that the idea of employing pressure in the axilla, as a means of counter extension, had long been abandoned by American surgeons, until revived by Dr Gibson in his modification of Hagedorn." This Dr Gibson wishes us to correct, as he declares it to be an error, and one, he remarks, which I have always taken great pains in my lectures to prevent the pupils from falling into, and have, moreover, stated explicitly the contrary in my text book, as the subjoined extracts will show.

"Counter extension then is made upon the *acetabulum of the sound side*, and extension upon the ancle of the injured limb, which, so long as the two feet are kept upon the same level, cannot be shortened, provided rotation of the pelvis be prevented. This purpose is answered by extending the splints to the arm-pit on each side, and not with a view, as might be supposed, of producing counter extension from these points."—*Gibson's Surgery, vol. i, p. 349. Second Edition.*

MIAMI UNIVERSITY. MEDICAL DEPARTMENT.

The Board of Trustees of Miami University announce, that they have established in Cincinnati a Medical Department, which will go into operation the ensuing autumn.

The following gentlemen compose the Faculty:

DANIEL DRAKE, M.D. (Late Professor in Transylvania University and Jefferson Medical College,) Professor of the Institutes and Practice of Medicine, and Dean of the Faculty.

GEORGE M'CLELLAN, M.D. (Professor of Surgery in the Jefferson Medical College,) Professor of Anatomy and Physiology.

JOHN EBERLE, M.D. (Late Professor of Materia Medica in Jefferson Medical College,) Professor of Materia Medica and Botany.

JAMES M. STAUGHTON, M.D. (Late Professor of Surgery in the Columbia College,) Professor of Surgery.

JOHN F. HENRY, M.D. of Kentucky, Professor of Obstetrics and the diseases of Women and Children.

THOS. D. MITCHELL, M.D. of Philadelphia, Professor of Chemistry and Pharmacy.

JOSEPH N. M'DOWELL, M.D. of Cincinnati, Adjunct professor of Anatomy and Physiology.

We regret that unavoidable circumstances have prevented us from inserting in the present number a notice of the very valuable work of Dr Darrach on "The Anatomy of the Groin."

By a mistake, the name of Dr W. S. W. Ruschenberger was incorrectly printed in our last number, page 242.

Dr W. C. Brewster has been appointed by the Governor, Physician to the port of Philadelphia.

University of Maryland.—A vacancy having happened in the Professorship of Anatomy, in the University of Maryland, there will be a meeting of the trustees on the first Monday of June next, for the purpose of appointing a Professor to that Chair.

It is with regret, that we announce the death of Dr DE BUTTS, Professor of Chemistry in the University of Maryland, who died on the morning of the 5th of April, of Pneumonia.

In the Boston Medical and Surgical Journal, Vol. iv, No. 7, it is stated that "Dr. Ruan, a Parisian physician, in a communication to Professor Hufeland of Berlin, states that he has succeeded in (relieving) some distressing cases of incessant itching of parts of the body covered with skin of very delicate structure, by the exhibition of the balsam copaiva."

Now the fact is, Dr Ruan is a highly respectable practitioner of Philadelphia, and his communication on the efficacy of balsam copaiva, in pruritis of the vulva, appeared originally in a former number of this Journal.

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